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"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Polit.* lib. i. cap. 1. Not.

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"Meditationis est perscrutari occulta; contemplationis est admirari perspicua . . . . . . Admiratio generat quæstionem, quæstio investigationem, investigatio inventionem."—*Hugo de S. Victore.*

—"Cur spirent venti, cur terra dehiscat,
Cur mare turgescat, pelago cur tantus amaror,
Cur caput obscura Phœbus ferrugine condat,
Quid toties diros cogat flagrare cometas;
Quid pariat nubes, veniant cur fulmina cœlo,
Quo micet igne Iris, superos quis conciat orbes
Tam vario motu."

J. B. Pinelli ad Mazonium.

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Errata.

P. 237, equation (a.), *for* $\frac{cdS}{2\lambda^r}$ *read* $\frac{cdS}{2\lambda r}$.

— 238, line 1, *for* line in ζ *read* sine in ζ.

— 239, — 10, *for* little less *read* a little less.

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[THIRD SERIES.]

JANUARY 1850.

I. *Experiments on the Extraction of Gold and Silver from their Ores by the Wet Way.* *By* John Percy, *M.D., F.R.S.**

ATTENTION has of late been directed to the extraction of silver by the *wet way* from many of its ores, of which large quantities are now imported into this country, especially from South America. Several processes have been projected; and one, the subject of a patent, is now extensively practised. It consists in calcining the ore with chloride of sodium, precisely as in the amalgamation process of Freiberg, and then dissolving out the chloride of silver formed during the calcination by a hot saturated solution of chloride of sodium. From this solution the silver is precipitated by metallic copper, and then cupelled. I have examined many South American silver ores, and have found them very variable in respect to the proportion of silver, and the presence of gold and other metals. In one ore I have found as much as $30\frac{1}{4}$ per cent. of fine silver. Several smelting establishments have, I know, attempted to work some of these ores in the dry way, but the results have not been favourable. Hence will appear the importance of directing attention to the œconomic extraction of the silver by the wet way.

The ore which I subjected to experiment was an auriferous silver ore, which contained a large proportion of blende, with galena, iron pyrites and copper pyrites in small quantity; the non-metallic part of it consisted chiefly of silica. The silver was present as sulphuret. By roasting, the ore swelled up much, and sulphurous acid was copiously disengaged. I received it in the state of coarse brownish-gray powder. I made numerous experiments upon it, to ascertain the best mode of extracting the precious metals by

* Communicated by the Author, having been read before the Chemical Section of the British Association at Swansea, 1848.

the dry way, and then proceeded with experiments on the extraction of those metals by the wet way, the results of which I now communicate.

The mean of two assays of the roasted ore by the dry way gave of fine silver containing gold—

7·977 grs. in 1000.
55·839 in the pound avoirdupois.
$260\frac{4}{8}$ oz. in the ton.

The silver contained of gold per cent. 3·78.

Therefore, the total quantity of fine silver in 1000 grs. of roasted ore is 7·676, and of fine gold 0·301.

Or, $250\frac{6}{8}$ oz. fine silver in the ton, and 9 oz. 16 dwts. fine gold.

1. Ammonia extracted some silver from the roasted ore, notwithstanding the roasting towards the last was effected at a bright red heat.

2. A solution of hyposulphite of soda also extracted from it a very sensible quantity of silver.

3. I digested 1000 grs. of roasted ore for a few hours with a dilute solution of hyposulphite of soda, containing 150 grs. of the crystallized salt. The digestion was made at a temperature just warm to the hand. I filtered, washed well, added to the filtrate an excess of dilute sulphuric acid, and then boiled. Almost immediately after the addition of the sulphuric acid, sulphurous acid was evolved and sulphur deposited; but until the solution became hot, it did not acquire a dark colour. I boiled for some time, stirring constantly until the supernatant liquor became bright and the dark precipitate subsided. I filtered, washed well and dried. I enclosed this precipitate in a sheet of assay-lead*, and cupelled. I obtained a bead of fine silver weighing 4·800. An accident occurred in this experiment, which may have caused a slight loss. I treated the bead with nitric acid, and obtained a sensible residue of gold. I parted, with the usual precautions, and melted the residue of gold before the blowpipe. The bead of fine gold weighed 0·009. The silver therefore contained 0·187 per cent. of gold. This result appeared to me to be interesting, as showing that hyposulphite of soda dissolved out a sensible amount of gold from the ore roasted *per se*. Is it probable that, in the presence of a large excess of other metallic oxides, the gold itself may, at least in a certain proportion, have existed in combination with oxygen? †

* Lead free from silver.

† See a curious paper by Brandt, *Recueil des Mémoires les plus intéressants de Chymie, &c., dans les Actes de l'Académie d'Upsal, &c.* Paris 1764. vol. ii. p. 357.

I treated the residual ore a second time with a dilute solution of hyposulphite of soda, and proceeded in the manner just described. I obtained another bead of fine silver, weighing 0·050. Some loss was also again accidentally occasioned in this second determination.

4. Goldleaf was digested in a solution of hyposulphite of soda, but it did not appear to be in any degree acted upon; neither did silver leaf appear to be appreciably dissolved by the same menstruum, although the surface after some time became tarnished, the solution being freely exposed to the atmosphere. A piece of copper wire, which had been previously dipped in nitric acid and washed, was immersed in the solution of hyposulphite containing the silver leaf; but there was not the slightest appearance of the deposition of silver. However, the surface of the copper acquired a dark colour, and by subsequent exposure to the atmosphere became beautifully iridescent.

5. Into a large stoppered bottle I put 1000 grs. of the roasted, finely pounded and sifted ore. I then nearly filled the bottle with distilled water, and passed a stream of chlorine through for an hour, having previously added 50 grs. of chloride of potassium, with a view to form a stable double salt of gold. After shaking the bottle at intervals during the passage of the gas, there was a manifest absorption of the gas, as proved by the rushing in of the air on withdrawing the stopper. The agitation was repeated several times, and with the same effect. I left the whole for about four days, shaking at intervals. When the powder had subsided after shaking, I uniformly observed the disengagement of numerous small bubbles of gas from the ore. On plunging a spill, burning only with a spark, into the neck of the bottle, combustion was evidently increased, the spark burning distinctly brighter. A strong odour of some oxygen compound of chlorine continued to be evolved. I filtered, and obtained a clear and colourless solution, which I reduced considerably by evaporation. During the evaporation, a white scum formed on the surface, and white matter ($PbCl$) was deposited, which towards the last occasioned succussions. To the solution thus reduced, when cold, 100 grs. of hyposulphite of soda were added; but the deposited matter did not appear sensibly to dissolve. I treated the residual ore with a solution of 200 grs. of hyposulphite of soda; I filtered and washed. I mixed the filtrate with the solution previously mentioned, and carefully washed in the precipitate. I added hydrochloric acid, and digested as usual, stirring continually. Sulphurous acid did not appear to be copiously evolved. The precipitate thus obtained was reddish-brown and not black. It was washed on a filter, dried,

enclosed in 130 grs. of assay-lead, and cupelled. The bead not being clean, was recupelled. I added 100 grs. of hyposulphite to the filtered solution and boiled, when a dark olive-brown precipitate was thrown down, which was dried, detached from the filter (the same as used in the first filtration) as completely as possible, and heated in a porcelain capsule to expel the free sulphur. A small quantity of black matter remained, which was enclosed in about 80 grs. of assay lead, and cupelled. When the lead was melted, I put the filter on it and incinerated, and I also added the bead of silver previously obtained, and enclosed in a small piece of assay lead. I obtained a good bead of fine silver. However, the cupellation was not perfectly satisfactory, as I observed some very minute particles of silver surrounded by black scoria on the sides of the cupel.

The bead weighed 5·385 grs. By parting with nitric acid, it yielded of gold 0·100, or 1·857 per cent.

6. 1000 grs. of the roasted ore were treated with chlorine precisely in the manner described in experiment 5, and left during four months. Bubbles of gas continued to be evolved sensibly for a long time; and even at the end of the four months, on stirring the ore, still continued to be disengaged from its surface. The clear supernatant solution was decanted, and the residue treated with a cold solution of 200 grs. of hyposulphite of soda; it was left for two or three days, filtered and washed with cold water. I then digested the filtrate on the sand-bath with excess of sulphuric acid until the odour of sulphurous acid ceased to be detected. I washed the precipitate with boiling water, when after long washing, it still continued to become turbid by the addition of BaCl (from PbCl). The dry precipitate was seen to be mixed with shining, colourless, acicular crystals. I roasted till no odour of sulphurous acid was evolved, and then cupelled the residue with 200 grs. of lead. There was evidently a considerable quantity of matter present which would not pass into the cupel. I recupelled the bead with a small additional quantity of lead.

I obtained a bead of fine silver, weighing 5·80 grs. On the addition of carbonate of potash to the decanted supernatant liquor, a copious white precipitate was thrown down.

7. I digested 1000 grs. of the finely-pounded roasted ore with a solution of hyposulphite of soda containing 200 grs. of the salt, at a temperature just warm to the hand, and then filtered. I boiled the filtrate, which became brown. I added to the boiling solution excess of dilute sulphuric acid, and continued to digest for some time at about 212° F., stirring continually. I filtered and dried. I detached the precipitate from the fil-

ter, and expelled the excess of sulphur by a gentle heat. I enclosed the residuum in about 100 grs. of assay lead, and cupelled. The bead weighed 0·802 gr. This is a very much smaller quantity than that previously obtained (exp. 3) by treating the roasted ore in the state of coarse powder without *subsequent* trituration, as was practised in the present experiment. Is it probable that by the act of trituration the associated matter present, which existed in so large a proportion compared with that of silver, may have protected the silver from the action of the hyposulphite?

On adding sulphuretted hydrogen water to the filtrate, after precipitation by sulphuric acid, not the slightest blackness was produced. A precipitate of the pure colour of sulphur, canary-yellow, was produced from the reaction of the sulphuretted hydrogen upon the sulphurous acid remaining in the solution. I transferred the ore, after treatment with the salt of soda, into a glass jar, and passed through a stream of chlorine for nearly an hour, stirred well, and left the jar with its contents covered until the following morning. The odour of chlorine still continued to be strongly evolved. I added chloride of sodium, and digested for several hours on the sand-bath, replacing from time to time the water removed by evaporation: the surface became covered with small white crystals. Afterwards I added a solution containing 200 grs. of hyposulphite of soda, and digested at a temperature just warm to the hand, stirring well. I filtered and washed, and again treated the residual ore with a further solution of hyposulphite containing 50 grs. of the salt. I then filtered and washed. I mixed the two solutions, and digested as usual with dilute SO^3 in excess, and filtered. On the addition of HS water to the filtrate, the latter became dark-coloured. I then added an excess of HS water until the odour of HS persisted, when after standing the filtrate did not again become discoloured. I washed and dried. The filtrate, which was readily detached from the filter without sensible loss, was gently heated in a porcelain capsule to expel the free sulphur. It then weighed 24 grs. I enclosed it, together with the ash of the filter, which was incinerated in the same capsule as used for expelling the free sulphur, in 200 grs. of assay-lead. The capsule was not stained, and every appreciable trace of matter was removed. By cupellation I obtained a bead weighing 5·005 grs.

I again exposed the residual ore diffused through water to the action of a stream of chlorine precisely as before, except that after digestion during a few hours I added KO until an alkaline reaction was obtained, the reaction having been found to be acid. White matter, apparently similar to that pre-

viously observed under the same circumstances, again occurred on the surface. I treated with a solution of 200 grs. of hyposulphite of soda. On testing the wash-water with HS water, S was precipitated, and there was not the slightest dark discoloration. I proceeded exactly as before, and cupelled with about 60 grs. of assay lead. The bead of fine silver weighed 0·675 gr. I again treated the residual ore in a similar manner with Cl for about an hour and a half, and then digested for a considerable time on the sand-bath. On the following day I dissolved out with a solution of 200 grs. of hyposulphite. I treated the filtrate as usual with dilute SO^3, and tested the wash-water with HS water. I proceeded as before. I employed 100 grs. of assay lead for cupellation. The bead of fine silver weighed 0·154. The total amount of fine silver obtained is 6·636 grs. By parting with NO^5, I obtained of gold 0·197. The silver therefore contained of gold 2·968 per cent.

I now boiled the residual ore with a solution containing 100 grs. of KCy, filtered and washed. By addition of HCl in excess to the filtrate, a white precipitate was produced; but on continuing the digestion until the odour of hydrocyanic acid could no longer be detected, it became dark-coloured, which I believed to be owing to the presence of S in some form. I digested the precipitate with NO^5, and obtained a pale blue solution, which did not become in the slightest degree turbid by the addition of HCl. On the filter remained only a minute quantity of matter, like sulphur in appearance, somewhat discoloured; and by heat the characteristic odour of sulphur was evolved, and only a minute quantity of matter was left.

8. In another experiment, in which I heated 1000 grs. of the *finely*-pounded ore, triturated after roasting, with a solution of 200 grs. of hyposulphite, I obtained a bead of fine silver weighing only 0·81.

9. I digested 1000 grs. of roasted ore with a solution of common chloride of lime (hypochlorite) on the sand-bath during two or three days. I then washed and digested the residuum, at a *warm* temperature, with a solution of 150 grs. of hyposulphite of soda. I filtered and washed. To the filtrate I added an excess of dilute SO^3, and heated till the supernatant liquor became quite clear. I filtered, washed slightly and dried. I enclosed the filter, with its contents, in 200 grs. of assay lead, and I subsequently added on the cupel 100 grs. I obtained a bead of fine silver weighing 5·469, and containing gold, but only to the extent of ·02. I do not, however, place much confidence in this determination of the

gold, as the manipulation was hardly conducted with the requisite care.

I dried the residual ore, and by trituration reduced it to a very fine state of division, so that it was passed through the finest copper sieve. I suspended it in cold distilled water, and passed through a stream of Cl during about a quarter of an hour, stirring continually. I then digested on the sand-bath, and left till the next day, when I still perceived a faint odour of Cl. I added 100 grs. of hyposulphite of soda, and digested at a gentle temperature, stirring occasionally. I decanted and filtered the supernatant liquor. I again digested the residue with an additional quantity of 100 grs. of hyposulphite, filtered and washed. I boiled the filtrate, as usual, with an excess of dilute SO^3. The precipitate soon became dark-coloured. I digested until the supernatant liquor became clear, filtered, washed and dried. I detached the precipitate from the filter, which was done without appreciable loss; heated it in a capsule, to expel the free sulphur, and digested it with aqua regia. As the white product which was obtained by this means was mixed with minute acicular crystals like chloride of lead, I treated it, after washing, with zinc and dilute hydrochloric acid. I washed and dried the metallic residue, enclosed it in 110 grs. of assay lead, and cupelled. I obtained a bead of fine silver weighing 0·829 gr., making a total of fine silver 6·298.

I examined the liquid left after digestion of the first precipitate with aqua regia, for gold. I diluted it considerably, and boiled with oxalic acid. On the following day (the oxalic acid being added at night) I observed a film of reduced gold of a reddish-brown colour; and a few days afterwards I distinctly recognised minute particles having the colour and metallic lustre of gold.

10. I treated 1000 grs. of the well-roasted ore, in its coarsely pounded state, with a cold solution of $Fe^2 Cl^3$. $Fe^2 O^3$ was soon precipitated, being displaced doubtless by the oxide of zinc *. The supernatant liquor became clear and colourless. I added HCl, which redissolved the $Fe^2 Cl^3$, but it was again precipitated. I added a considerable quantity of HCl, and obtained a greenish-yellow brown solution. I filtered and washed, and treated the residue with a solution of 150 grs. of hyposulphite of soda. I proceeded as uusul with SO^3. I used 200 grs. of assay lead in the cupellation, and obtained a bead of fine silver weighing 4·909 grs.

11. I confirmed the fact, that when chloride of silver is dissolved in a solution of hyposulphite of soda, and treated with a

* Oxide of copper will also displace it.

mineral acid, a copious precipitation of dark-coloured matter takes place, which is doubtless AgS. But whether the whole of the silver is precipitated under these circumstances as sulphuret I did not ascertain; though from the copious precipitate produced, I think that such is probably the case.

Concluding Observations.

1. The loss of fine silver in the most satisfactory experiment was $13\frac{1}{2}$ per cent. Formerly, in the old amalgamation process in Mexico, my respected friend Mr. John Taylor informs me, the loss frequently amounted to 35 per cent. But at present, since the introduction of the barrel-process, the loss has been reduced to 9 or even 5 per cent. My results therefore may be regarded as sufficiently satisfactory to justify attention to them with reference to their œconomic application. It is probable that on the large scale, with proper appliances, the silver would be much more completely extracted.

2. I would especially direct attention to chloride of lime and chlorine as the agents for effecting the conversion of the silver into chloride, and to hyposulphite of lime, which may readily be obtained as a cheap substitute for hyposulphite of soda to dissolve the chloride. The silver it is obvious might be precipitated either as metal or sulphuret.

3. Since many of the South American silver ores contain gold, it is desirable that both the silver and gold should be extracted by one process; and to this end chlorine or chloride of lime seems to be indicated by the preceding experiments.

II. *On Annuities and Assurances on Successive Lives.* *By* THOMAS WEDDLE, *F.R.A.S.**

MILNE, in his Treatise on Annuities (Chapter VII.), has discussed this subject with much clearness and elegance; yet his method of investigation does not seem to be the best possible, nor do I think that his results are given in forms well adapted to numerical applications. It may therefore be not altogether useless to consider the subject in a somewhat different manner, and so to exhibit the formulas as best to meet the wants of the computer.

Problem I. To determine the present value (a_n) of an assurance of £1 payable on the failure of the last of n successive lives A', A'' .... $A^{(n)}$.

Let $a^{(p)}$ denote the value at the time of nomination of an assurance of £1 payable on the death of $A^{(p)}$; $A^{(p)}$ itself de-

* Communicated by the Author.

noting the value of an annuity of £1 during the life of $A^{(p)}$, so that we have (r being the interest of £1 for a year)

$$a^{(p)} = \frac{1-rA^{(p)}}{1+r}; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

$$\therefore \ 1-a^{(p)} = \frac{r}{1+r}(1+A^{(p)}), \quad . \quad . \quad . \quad . \quad . \quad . \quad (2.)$$

and

$$1+A^{(p)} = \left(1+\frac{1}{r}\right)(1-a^{(p)}). \quad . \quad . \quad . \quad . \quad . \quad . \quad (3.)$$

We have evidently $a_{\prime}=a'$; also when A'' shall be nominated the value of £1 payable at his death will be a''; but the present value of £1 payable when A'' shall be nominated, that is, at the end of the year in which A' shall die, is $a_{\prime}$; hence the present value of £1 payable at the death of A'' is

$$a \times a'' = a'a'';$$
$$\therefore \quad a_2 = a'a''.$$

And generally when the life $A^{(p)}$ shall be nominated, the then value of £1 payable at his death will be $a^{(p)}$; but the present value of £1 payable at the death of $A^{(p-1)}$, that is, at the nomination of $A_{(p)}$, is a_{p-1},

$$\therefore \quad a_p = a_{p-1} \, . \, a^{(p)}.$$

Hence taking $p=1, 2, 3 \ldots . n$ in succession, we have

$$\begin{aligned} a_1 &= a' \\ a_2 &= a_1 a'' \\ a_3 &= a_2 a''' \\ &\vdots \\ a_n &= a_{n-1} \, . \, a^{(n)}. \end{aligned}$$

Multiply these equations together and cancel the common factors

$$a_n = a'a''a''' \ldots \ldots a^{(n)} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4.)$$

Hence the present value of an assurance payable on the failure of the last of any number of successive lives is very readily computed, providing we have a table of the present values of assurances on single lives; and such tables are given in D. Jones's work on Annuities and Reversionary Payments (Tables IX. and XXII.), according to both the Northampton and Carlisle Tables of Mortality. If however only a table of annuities be at hand, it will be better to modify (4.) as follows:—

By (1.) we have $\mathrm{a}^{(p)}=v(1-r\mathrm{A}^{(p)})$, where $v=(1+r)^{-1}$, the present value of £1, due in a year, hence (4.) becomes

$$\mathrm{a}_n=v^n(1-r\mathrm{A}')\,.\,(1-r\mathrm{A}'')\,.\,.\,.\,.\,.\,(1-r\mathrm{A}^{(n)})\,.\,.\quad . \quad . \qquad (5.)$$

Problem II. To determine (a_p) the present value of an annuity of £1 on the pth life mentioned in Problem I.

At the nomination of $\mathrm{A}^{(p)}$ £1 will be due, and the annuity on his life will be worth $\mathrm{A}^{(p)}$; hence the life will then be worth $1+\mathrm{A}^{(p)}$; but £1 due at the nomination of $\mathrm{A}^{(p)}$ is now worth a_{p-1}; hence the present value of the annuity on the pth life is

$$\mathrm{a}_{p-1}\,.\,(1+\mathrm{A}^{(p)}),$$

$$\therefore,\ (4.),\ a_p=\mathrm{a}'\mathrm{a}''\ldots\mathrm{a}^{(p-1)}\,.\,(1+\mathrm{A}^{(p)}),\ .\ \ .\ .\ .\ .\qquad (6.)$$

and this is the present value of £1 per annum on the pth life.

The preceding (6.) is the form that will generally be found best adapted to computation; but a_p may also be expressed in terms of a only, or of A only, as follows (see (1.) and (3.)):—

$$a_p=\left(1+\frac{1}{r}\right)\mathrm{a}'\mathrm{a}''\ldots.\mathrm{a}^{(p-1)}(1-\mathrm{a}^{(p)}),\ .\ .\ .\ .\ .\ .\ .\qquad (7.)$$

and

$$a_p=v^{p-1}(1-r\mathrm{A}')(1-r\mathrm{A}'')\ldots.(1-r\mathrm{A}^{(p-1)})(1+\mathrm{A}^{(p)})\,.\qquad (8.)$$

It may be observed too that (7.) is little, if any, inferior to (6.) in point of easy application, as it requires a reference to one table only, while (6.) requires a reference to two.

Note. Unless the annuity be *due*, that is, unless a payment is to be made immediately, none of the preceding formulas will give a_1, the value of the annuity on the first life, correctly. If we suppose, as usual, that the first payment will be made in a year, the true value of a_1 will be

$$a_1=\mathrm{A}'=\left(1+\frac{1}{r}\right)(1-\mathrm{a}')-1,\ .\ .\ .\ .\ .\ .\ .\ .\qquad (9.)$$

and not

$$a_1=1+\mathrm{A}'=\left(1+\frac{1}{r}\right)(1-\mathrm{a}').$$

Problem III. To determine (A_n), the present value of an annuity of £1 on the n successive lives $\mathrm{A}', \mathrm{A}''\ldots.\mathrm{A}^{(n)}$.

We evidently have

$$\mathrm{A}_n=a_1+a_2+a_3\ldots.+a_n.$$

But (7.) and (9.)—

$$a_1 = \left(1+\frac{1}{r}\right)(1-\mathrm{a}')-1$$

$$a_2 = \left(1+\frac{1}{r}\right)(\mathrm{a}'-\mathrm{a}'\mathrm{a}'')$$

$$a_3 = \left(1+\frac{1}{r}\right)(\mathrm{a}'\mathrm{a}''-\mathrm{a}'\mathrm{a}''\mathrm{a}''')$$

$$\vdots$$

$$a_n = \left(1+\frac{1}{r}\right)(\mathrm{a}'\mathrm{a}''\ldots\mathrm{a}^{(n-1)}-\mathrm{a}'\mathrm{a}''\ldots\mathrm{a}^{(n)}).$$

Hence by addition we have

$$\mathrm{A}_n = \left(1+\frac{1}{r}\right)(1-\mathrm{a}'\mathrm{a}''\ldots\mathrm{a}^{(n)})-1 \quad . \quad . \quad . \quad . \quad . \quad (10.)$$

It appears from (4.) that (10.) is equivalent to

$$\mathrm{A}_n = \left(1+\frac{1}{r}\right)(1-\mathrm{a}_n)-1;$$

and, in fact, this might have been deduced immediately from (3.), for the formula (1.) which expresses the relation between an annuity and the corresponding assurance, is true of any status*, providing the status be such that the assurance on it *must* be paid some time or other.

Problem IV. A copyhold estate is held on a certain number of lives A, B, C .... and each life is renewable at the end of the year in which it may fail by paying a fine of f pounds. Required the present value of all the fines.

Let A′, A″ .... be the lives that succeed A; B′, B″ .... those that succeed B, &c.; also let a, a′, a″ .... be the values of assurances on the lives A, A′, A″ .....; b, b′, b″ .... those on B, B′, B″ ...., &c.

By (4.) it appears that the fines (each equal to f) payable at the deaths of A, A′, A″ .... are now worth fa, $f$aa′, $f$aa′a″, ... respectively; and similar expressions being true of the other successive lives, the present value of all the fines is,

$$f \times \left\{\begin{array}{l} \mathrm{a}+\mathrm{aa}'+\mathrm{aa}'\mathrm{a}''+\ldots\ldots \\ +\mathrm{b}+\mathrm{bb}'+\mathrm{bb}'\mathrm{b}''+\ldots\ldots \\ +\mathrm{c}+\mathrm{cc}'+\mathrm{cc}'\mathrm{c}''+\ldots\ldots \\ +\ldots\ldots\ldots\ldots\ldots\ldots \end{array}\right\} \quad . \quad . \quad . \quad . \quad . \quad (11.)$$

* "By the *status* of an annuity, I mean the state or condition of things during the continuance of which the annuity is to be paid."—De Morgan's *Essay on Probabilities*, p. 190.

If the lives, at the time of the nomination, be all of the same age (which must be assumed in practice), then, P denoting the value at the time of nomination of an annuity of £1 on each renewal life, and p the assurance on the same, we shall have

$$a' = a'' = \ldots\ldots = b' = b'' = \ldots\ldots\ldots = c' = c'' = \ldots\ldots\ldots\ldots = p,$$

and each horizontal row in (11.) now constitutes a geometrical progression of which the common ratio is p; and the sum of the whole is evidently

$$f.\left\{\frac{a}{1-p}+\frac{b}{1-p}+\frac{c}{1-p}+\ldots\right\}$$

Hence

$$f.\frac{a+b+c+\ldots\ldots\ldots}{1-p}, \quad . \quad . \quad . \quad . \quad . \quad (12.)$$

is the present value of all the fines.

The preceding expression (12.) may also be exhibited in terms of the annuities on the lives instead of the assurances on the same. For

$$(1.),\ a=\frac{1-rA}{1+r},\ b=\frac{1-rB}{1+r},\ldots, \text{and, } (2.),\ 1-p=\frac{r}{1+r}(1+P),$$

hence (12.) becomes

$$f.\frac{n\frac{1}{r}-(A+B+C+\ldots)}{1+P}, \quad . \quad . \quad . \quad . \quad . \quad (13.)$$

where n denotes the number of lives A, B, C... on which the estate is held, and it may be observed that $\frac{1}{r}$ is a perpetuity of £1.

The formulas (6.), (10), (11.) and (12.) agree with those given by Mr. Milne, but they are here expressed in a way better adapted to computation. That these coincide with Mr. Milne's expressions, will readily appear, if instead of A'.... we introduce annuities certain of equal values; thus let $A^{(p)}$ be equivalent to an annuity certain for t_p years, so that

$$A^{(p)}=\frac{1-v^{t_p}}{r},$$

$$\therefore 1+A^{(p)}=\left(1+\frac{1}{r}\right)(1-v^{t_p+1}), \text{ and, } (2.),\ a^{(p)}=v^{t_p+1},$$

hence (10.) becomes

$$A_n=\left(1+\frac{1}{r}\right)(1-v^{\sigma+1})-1,\ (\sigma=n-1+t_1+t_2\ldots+t_n),$$

or

$$A_n = \frac{1-v^{\sigma}}{r},$$

which is Mr. Milne's formula; and similarly, it may be shown that (6.) and (11.) coincide with the expressions given in Milne.

Also if P be equivalent to an annuity certain of t years, so that $P=\frac{1-v^t}{r}$, and therefore $p=v^{t+1}$, then (12.) becomes

$$f\frac{a+b+c+\ldots}{1-v^{t+1}},$$

which is Mr. Milne's expression.

The formula (13.) is not in Milne, but is given by Professor De Morgan in his Essay on Probabilities (Cab. Cyclop.), Appendix the Second. It is very well adapted to computation, though not I think quite so well as (12.), if Jones's tables previously alluded to be used. The difference between the two formulas in point of practical application is however very trifling.

III. *On the supposed Inversion of Hydrostatical Principles which takes place in the casting of Specula for Reflecting Telescopes on a "chilling" base formed of hoop-iron packed edgewise. By* Professor POTTER, *A.M.**

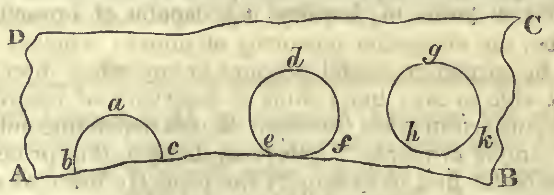
THE method of forming the "chilling" base of the mould for casting specula with hoop-iron packed edgewise, instead of a simple disc of iron, is Lord Rosse's undisputed discovery. In his paper in the Philosophical Transactions for 1840, entitled "An account of Experiments on the Reflecting Telescope. By the Right Honourable Lord Oxmantown, F.R.S," he has the following:—"At first a simple disc of iron was tried, but although the castings were sound, there was this defect; that bubbles of air were often entangled between the iron and speculum metal, producing cavities which it was tedious to grind out: the iron disc was therefore replaced by one made of pieces of hoop-iron placed side by side, with their edges up, tightly packed in an iron frame: the edges were brought to a smooth surface of the proper curve either by the file or lathe, whichever was the most convenient. A metallic surface was thus constructed everywhere porous; as however close the hoop-iron had been packed, the interstices suffered air to pass freely through. So successful was this

* Communicated by the Author.

expedient, that of sixteen plates cast for the three-feet speculum, not one was defective:" &c.

If the successful casting of these sixteen plates was due to the base being of packed hoop-iron in place of a solid disc, it must be admitted that Lord Rosse's discovery was a great improvement upon the solid disc; but if the ordinary principles of hydrostatics show that such an effect could not result, the cavities which were found on the surface of his first castings upon a solid disc must be referred to some other cause; such as, that in his first attempts on the "chilling" method, the melted metal not being sufficiently fluid for that mode of casting, it did not flow uniformly over the disc, but becoming solid too quickly, left cavities in some places.

If the metal retained its fluidity for ever so brief an interval of time after an air-bubble was entangled between the iron and melted speculum metal, we can ascertain the nature and effect of the forces acting on it by the principles of hydrostatics. Let AB be the upper surface of the chilling body, *bac*



a portion of air entangled within the melted metal ABCD. Now the pressure at different points in a fluid increasing with the depth, the parts at *b* and *c* will be subject to greater pressure than those about *a*; and hence the bubble of air will instantly take the form *edf*, and ascend as at *hgk* through the fluid metal, like air-bubbles in water, by virtue of the property of fluid pressure, that "*the resultant pressure of a fluid, on a body immersed in it, equals the weight of the fluid displaced, and acts vertically upwards through the centre of gravity of the fluid displaced.*" It is hence futile to prepare a way for escape downwards when there is no tendency to escape in that direction.

As fluids in contact, which do not mix, arrange themselves in descending order according to their greater specific gravities, it is impossible to see how air could place itself below *fluid* metal; and we must conclude that the presence of cavities on the face of his castings indicate that his Lordship's metal was then deficient in fluidity, and, flowing unequally over the chilling surface, on becoming too quickly solidified, contained the cavities which were found.

It is perhaps needless to remark that air-holes are left through the upper part of the sand of the mould to allow a free escape of the air upwards, as it is displaced by the metal entering by the ingate.

My apology for writing in so much detail on such a subject is, that I am well-aware the number of amateur telescope-makers in this country is much greater than generally imagined; and one who immediately published his discoveries for the benefit of brother amateurs, ought also to caution them against useless and unphilosophical methods.

IV. *On a property of the Interminable Decimals which arise from the development of an Incommensurable Fraction.* By J. R. Young, late *Professor of Mathematics, Belfast**.

THE following curious property of fractional developments has, I believe, hitherto escaped notice. I find that, in at least one interesting inquiry, it is capable of a practical application, not altogether unworthy of notice: whether or not it can be turned to useful account in any other direction, I am not able to say; but I think it deserving of record, as a remarkable arithmetical theorem, though depending only upon a very simple principle. Before explaining this principle, it may be better first to exemplify the property itself, as a thing ceases to appear remarkable, and often ceases to be valued, as soon as it is found to be capable of a simple explanation.

Let an incommensurable fraction be taken at random, say $\frac{1}{7}$; then

$$\frac{1}{7} = \cdot 142857142857142857 1428, \&c.$$

It is a property of the development of such a fraction that, stop wherever we may, a certain number of figures may be cut off the end, such that if the figures left be all multiplied by a certain determinable number, all the original figures will be reproduced, with the exception of a limited number of the *leading* figures. In performing the multiplication here mentioned, account must, as usual, be taken of the *carryings* from the figures cut off.

Cut off *one* place from the foregoing decimal, and multiply the whole row, omitting that one, by 3; arranging the result underneath, so that the like reproduced figures may stand

* Communicated by the Author.

under those above: we thus have

$$\frac{1}{7} = \cdot 1428571428571428571 42\ 8$$
$$428571428571428571 42\ 8$$

Cut off *two* places, and multiply by 2:

$$\cdot 1428571428571428571 4\ 28$$
$$2857142857142857 14\ 28$$

Cut off *three* places, and multiply by 6:

$$\cdot 142857142857142857 1\ 428$$
$$857142857142857 1\ 428$$

In like manner, cut off *four* places, and multiply by 4; *five* places, and multiply by 5; *six* places, and multiply by 8; *seven* places, and multiply by 3; *eight* places, and multiply by 9, or by 2; *nine* places, and multiply by 6. And in all these cases the decimals will recur as above, to whatever extent the development be carried.

Similar circumstances have place for all incommensurable fractions, the multipliers being different for different fractions. Thus, take $\frac{1}{13}$.

$$\frac{1}{13} = \cdot 076923076923076923076.$$

If two figures be cut off, the proper multiplier will be 9; if three be cut off, it will be 12; if *four* be cut off, it will be 3; if *five*, 4, &c.

I proceed to show what I venture to think to be a useful application of the above property.

In finding the circumference of a circle from the diameter, the usual process is to multiply the diameter by the number 3·1416; and, in determining the diameter from the circumference, to divide by the same number. When the diameter consists of several figures, the multiplication is rather long; and there is a proportionate liability to error, especially in the *addition* part of the work. And when the circumference is given, whether it be a large number or a small one, the division becomes tedious if several decimals are required. For rough purposes, the ratio $\frac{22}{7}$ of Archimedes is employed, as involving simpler operations. I propose here to show how the rule of Archimedes may be perfected by aid of the above property.

$$\frac{22}{7} = 3\cdot14285714\ 2857\ \&c.,$$

Multiply by 4, and subtract: 125714 2857

there remains 3·1416

We have thus got the number employed in the more accurate approximation; so that we have the following easy rule.

Multiply the diameter by 22, divide the product by 7, cut off *four* figures on the right, multiply the others by 4, place the product under the unabridged number, and *subtract*: the remainder will be the circumference.

Multiply the circumference by 7, divide the product by 2×11, cut off *four* figures on the right, multiply the others by 4, placing the product under the unabridged number, and *add*: the sum will be the diameter.

Ex. 1. The diameter of a circle is 883220 miles: required the circumference.

```
     883220
         22
    -------
    1766440
   1766440
  ---------
 7|19430840
  ---------
    2775834· 2857
       1110· 3337
    -------------
Circumference = 2774723· 952 miles.
```

And this result is the same as we should have got by employing the number 3·1416.

2. The circumference of a circle is 6850: required the diameter.

```
      6850
         7
   -------
 2|47950
   -------
11|23975
   -------
   2179·5 4545
        8 7182
   -----------
Diameter = 2180·4 1727
```

The foregoing principle is easily explained as follows:—

If we subtract a single decimal figure from unity, we shall get a single decimal figure for the remainder; if the subtractive decimal be preceded by a cipher, we shall get a single figure preceded by a ·9; if the subtractive decimal have two

ciphers prefixed, the remainder will be a figure preceded by two ·9s, and so on. Consequently, if unity be divided by any number, giving an interminable decimal, as by 7, for instance, then if from this unit we take care to subtract such a figure, preceded by ciphers or not, as will cause the remainder to be accurately divisible by 7, it is plain that the subtractive number when divided by 7, must furnish the same interminable decimals, after a limited number, as those furnished by $\frac{1}{7}$; otherwise the difference between the two rows could not be a finite number; that is, the remainder mentioned above could not be exactly divisible by 7.

This explanation will no doubt prove sufficient: *three* figures were cut off from the end of the partial development of $\frac{1}{7}$ above, and the other figures multiplied by 6, because $1 - {\cdot}006 = {\cdot}994$, which is exactly divisible by 7.

But an explanation yet more simple readily offers itself. Still reverting to the fraction $\frac{1}{7}$, we see that, in prosecuting the division, the first remainder that we get is ·3; therefore

$$\frac{1}{7} = {\cdot}1 + \frac{{\cdot}3}{7};$$

so that

$$\frac{1}{7} - \frac{{\cdot}3}{7} = {\cdot}1,$$

the first figure in the development of $\frac{1}{7}$. Again, the second remainder furnished is ·02, and

$$\frac{1}{7} - \frac{{\cdot}02}{7} = {\cdot}14,$$

the first two figures in the development. In like manner, the third remainder being ·006,

$$\frac{1}{7} - \frac{{\cdot}006}{7} = {\cdot}142,$$

the first three figures in the development; and, generally, the remainder carried from the nth figure of the development, being in the nth place of decimals, $\frac{1}{7}$, that is the whole development, diminished by $\frac{1}{7}$ multiplied by that remainder, must leave a result consisting only of the n first figures of the development. Hence the number n of figures to be cut off being

given, the proper multiplier is at once found by observing what is carried from the nth figure of the development; and it is plain that what is thus carried may be increased by any multiple of the divisor. And nothing further in the way of explanation can be required.

A word or two is however necessary, in reference to the foregoing rule, for finding the diameter of a circle from its circumference. By the rule, the expression for the diameter from the circumference, c, is

$$\frac{7}{22}(1+\cdot 0004)c.$$

But the true formula, or rather that which replaces the use of 3·1416, is

$$\frac{7}{22(1-\cdot 0004)}c=\frac{7}{22}\cdot\frac{1+\cdot 0004}{1-\cdot 00000016}c.$$

So that in the work of the second example above, the divisor $1-\cdot 00000016$ has been regarded as *unit*; an error which cannot affect the first six places of figures, and which can affect the seventh only by a unit; for

$$\frac{1+\cdot 0004}{1-\cdot 00000016}=\frac{(1+\cdot 0004)(1+\cdot 00000016)}{1-\cdot 0000000000000256}.$$

And this error is of no practical importance, in reference at least to a comparison between the new and the old rules; for the seventh figure will always be erroneous, even when 3·1416 is employed, the result can be correct only to the sixth place in the most favourable circumstances.

I would here further add, as arithmetical facility, in calculations of very frequent occurrence in practice, is my more especial object of consideration in the present paper, that the result of any multiplication by 3·1416 may with but little trouble be converted into what the more accurate multiplier 3·141593 would have produced. For since

	3·1416
diminished by	3·141593
is	·000007

it follows that if we only push the number multiplied by 3·1416 *six* places back, multiply it so depressed by 7, and subtract the result from the product furnished by 3·1416, we shall get the same figures as the more correct multiplier above would have produced. For instance, from the circumference determined above, namely,

	2774723·9520
subtract	6·182540
and we shall have	2774717·769460

the subtractive number being seven times the proposed diameter pushed *six* places to the right. And similar facilities may be introduced, in connexion with long multiplications, whenever the penultimate figure in the multiplier is a 9. I have only one other remark to make in reference to these simple matters. Should any one count the number of figures employed in the first example above, and compare the amount with the number of figures engaged in the common process, no saving will appear to have been effected; but that *time* is saved, and the risk of error diminished, by the more simple character of the operation proposed, cannot, I think, be questioned.

But *figures* may be saved. The above way of employing the multiplier $\frac{22}{7}$, is not the best way: the *best* way is that which follows, namely,

$$\begin{array}{r}
883220 \\
3\frac{1}{7} \\
\hline
2649660 \\
126174{\cdot}2857 \\
\hline
2775834{\cdot}2857 \\
1110{\cdot}3337 \\
\hline
2774723{\cdot}952
\end{array}$$

I need scarcely say, in conclusion, that the above principles, applied to the multiplier 3·1416, are in the same way applicable to ·7854, the fourth part of that multiplier: this may be replaced by the more simple multiplier $\frac{11}{2 \times 7}$, the above-explained correction being introduced.

The readers of this Journal will perhaps pardon me for obtruding upon their attention matters of so truly elementary a character. The composition of the paper has, for a few hours, relieved my mind from the pressure of a most painful and a most unmerited calamity; and as it is very probable that I shall not encumber these pages again for a long time, I crave their indulgence on the ground just stated.

London, Dec. 8, 1849.

[With sincere regret we advert to the circumstance to which our able and valued correspondent refers.—The Royal Belfast Academy, in which Mr. Young has with the highest reputation filled the Chair of Mathematics for sixteen years, having merged in the new College of Ulster, he has been superseded, and is now sent adrift upon the world, his library sold, or rather thrown away, and his large family

reduced to destitution.—Surely the long and able services of such a man, in an institution supported by a Parliamentary grant, must entitle him at least to compensation by a superannuation allowance. To the value of those services, and to his great ability as a writer, ample testimony has been given by the most eminent mathematicians, Dr. Peacock, Professors Challis, King, Sir D. Brewster, Sir W. R. Hamilton, Graves, Baden Powell, Sir J. W. Lubbock, Kelland, Christie, and De Morgan, whilst the whole of his late colleagues in the Academy, and all the clergy of Belfast have thus expressed their sense of his moral worth: "Amidst all the difficulties surrounding him as the head of a numerous young and therefore helpless family, we have reason to believe that his probity and correctness of conduct have ever remained irreproachable, nor have we at any time heard of the least moral taint attaching to his reputation. Under all these circumstances we deeply sympathize with Mr. Young, and so, we are satisfied, will the public in general."—Ed. Phil. Mag.]

V. *Mineralogical Notices.*

SCHORLAMITE $2(3RO+2SiO^3)+3(2RO+TiO^2)$.

BY M. RAMMELSBERG.

[Poggendorff's *Annalen*, vol. lxxvii. p. 123.]

THE hardness and specific gravity of this mineral from Magnet Cove, Hot Springs, County Arkansas, which Rammelsberg procured from M. Krantz of Berlin, were found to agree pretty well with the statements of Professor Shephard, the first being 7—7½, the latter 3·783. The chemical reactions however Rammelsberg found to differ considerably from those described by Shephard. When heated alone the mineral gave off nothing volatile; before the blowpipe in the platinum forceps it fuses with difficulty at the edges into a black mass; the borax bead is yellow in the outer flame, and becomes colourless on cooling, unless a very large quantity of the mineral has been dissolved; in the inner flame it appears green after treatment with tin. It likewise gives a yellow glass with microcosmic salt in the outer flame, which becomes colourless still more readily; treated with tin on charcoal in the inner flame, it at last becomes distinctly violet. The finely pulverized mineral is but imperfectly decomposed by hydrochloric acid. On ignition in a closed platinum crucible, no alteration in weight or colour results. Two analyses gave—

	I.	II.	Oxygen.	
Silicic acid . .	27·85	26·09	...	13·55
Titanic acid . .	15·32	17·36	...	6·74
Protoxide of iron	23·75	22·83	5·07	
Lime	32·01	31·12	8·85	14·53
Magnesia . . .	1·52	1·55	0·61	
	100·45	98·95		

CHLORITE. BY M. RAMMELSBERG.

[Poggendorff's *Annalen*, vol. lxxvii. p. 414.]

According to Rammelsberg, two minerals, Leuchtenbergite and Pennine, certainly belong to chlorite, and should not be separated from it by distinct names. A review of the various analyses which Rammelsberg has corrected and rendered more complete, has led him to the following conclusions respecting the Chlorites:—Certain varieties of Chlorites occur, which are characterized by containing about 30 per cent. of acid, and by a lesser amount of iron, such for instance are the Chlorites of Achmatowsk, from Schwarzenstein in Zillerthal, from Mauléon in the Pyrenees, from the river Balschoi Iremel, and from the Schischminskian Mountains near Slatoust; further, from Zermatt in the Matterthal Canton Vaux; and these two latter have been incorrectly distinguished as Leuchtenbergite and Pennine*. The relation of the oxygen to the magnesia or (FeO) of the alumina, or (Fe^2O^3) of the silica and of the water, is for all more or less nearly as 5 : 3 : 6 : 4, and the formula thence deducible is

$$(3RO + SiO^3 + R^2O^3 SiO^3 + 2HO) + 2MgO, HO.$$

It is however highly probable with respect to the other varieties, on account of the difficulty in determining the relative quantities of protoxide and peroxide of iron, that the above relation = 3 : 2 : 4 : 3. To the variety corresponding to this atomic proportion, Rammelsberg gives the name of Chlorite. Its formula is consequently

$$3(3RO + SiO^3) + (2R^2O^3 + SiO^3) + 9HO.$$

Ripidolite, $3(3RO + SiO^3) + (3R^2O^3 + SiO^3) + 9HO$, is the name which Rammelsberg assigns to the series of modifications which occur at St. Gothard, in the valley Rauris of the Pinzgau, at the Greiner in the Zillerthal, at St. Christoph, and at Mont des Sept Lacs in Savoy; they are characterized by a less amount of acid (26—27 per cent.), a less amount of magnesia, and a larger amount of iron, and appear all to possess the following relations as regards the oxygen, 3 : 3 : 4 : 3.

According to this therefore the Chlorites contain one-third less alumina than the Ripidolites, the quantities of the other elements remaining the same. Both series however are so alike in their properties, that if we suppose alumina to replace silicic acid in the Chlorites in the atomic proportion of 1 : 1, and in the Ripidolites of 3 : 2, a common expression is obtained for the two minerals, both then becoming hydrated bisilicates (aluminates).

* See Phil. Mag. vol. xviii. p. 121, and vol. xxi. p. 76.

GLAUKODOTE. BY MM. BREITHAUPT AND PLATTNER.

[Poggendorff's *Annalen*, vol. lxxvii. p. 127.]

Under this name Breithaupt describes a mineral from the district of Huasco, near Valparaiso in Chili, which furnishes a blue pigment. It has a metallic lustre, and is of a dark tin-white colour. Streak black; hardness 7; brittle; spec. grav. 5·975, 5·978, 6·003. Glaukodote occurs in seams in chloritic slate, accompanied by heavy Glanzcobalt (Cobaltine, Cobalt-glanz), and other minerals. According to Plattner, this mineral gives exactly the same reactions before the blowpipe as Cobaltine, and an analysis made by that chemist shows that Glaukodote contains the same per-centage of sulphur as the Cobaltine from Skutterrud, according to Stromeyer; the sum of the quantities of cobalt and iron in the two minerals likewise agree, but the atoms Fe, Co, S, As are in Glaukodote as 2 : 4 : 6 : 6, leading to the formula $2(CoS^2+CoAs^2)+(FeS^2+FeAs^2)$, which denotes a compound of $\frac{2}{3}$ Cobaltine and $\frac{1}{3}$ arsenical pyrites. The quantities found, compared with those calculated according to this formula, are—

Sulphur . . .	20·210	6 =	1200·0	19·40
Arsenic . . .	43·200	6	2812·5	45·46
Cobalt . . .	24·774	4	1474·4	23·83
Iron	11·900	2	700·0	11·31

EMBOLITE. BY MM. BREITHAUPT AND PLATTNER,

[Poggendorff's *Annnlen*, vol. lxxvii. p. 134.]

Is the name assigned by Breithaupt to a bromide of silver from the Colorada mine near Copiapo in Chili. It occurs in cubic crystals upon a very ferruginous limestone, is externally of an olive and asparagus green, but in the interior of a sulphur-yellow to finch-green colour. When recently extracted by smelting, it is likewise of a sulphur-colour. It has diamond lustre, hardness nearly 2, perfectly malleable, spec. grav. 5·806. Plattner found 5·789 for a yellow sample extracted by smelting, and 5·790 for a sample of green colour, and in another specimen even above 5·8.

Plattner's analysis gave—

Silver . . .	66·862	5	66·964
Bromine . .	20·088	2	19·841
Chlorine . .	13·050	3	13·195

leading to the formula $2AgBr+3AgCl$.

LONCHIDITE. BY M. BREITHAUPT.

[Poggendorff's *Annalen*, vol. lxxvii. p. 135.]

Is the name given by Breithaupt to the same mineral which he formerly described by that of Kansimkies; it has hitherto occurred but in small quantity in the Kurprinz Friederic August mine at Grosschirma, near Freiberg, always upon copper pyrites, accompanied by iron pyrites, spathic iron, horn-stone, quartz, &c.; also at the Sauschwart mine near Schneeberg, and likewise at Coak's Kitchen in Cornwall; also upon copper pyrites. It has a brilliant metallic lustre, is of a tin-white colour, sometimes variegated, and sometimes with a greenish iridescence. Streak black, differing in fact in nothing from the common arsenical pyrites as regards lustre, colour and streak. Hardness $=7\frac{1}{4}-7\frac{3}{4}$; spec. grav. 4·925, 4·936, 4·938, 4·962, 5·001. From the results of his analyses, Plattner declares Lonchidite to be a bisulphuret FeS^2, combined with some arsenical pyrites FeS^2+FeAs^2, in which latter however a part of the iron is replaced by copper and cobalt. His analysis gave—

Sulphur	49·612
Arsenic	4·396
Iron	44·225
Cobalt	0·354
Copper	0·749
Lead	0·204
	99·540

KONICHALCITE. BY MM. BREITHAUPT AND FRITZSCHE.

[Poggendorff's *Annalen*, vol. lxxvii. p. 139.]

This mineral was obtained from Spain, and has long been in the Freiberg collection. It has a slight vitreous lustre, a pistaceous emerald-green colour, with a similar coloured streak, and is transparent at the edges. Cleavage splintery, passing into fine granular. Brittle. Hardness $5\frac{1}{4}-5\frac{3}{4}$; spec. grav. $=4·123$, Breithaupt. The analysis is by Fritzsche. Reckoning the vanadic as phosphoric acid, the formula

$$2(3CuO+AsO^5)6CaO+PO^5+5HO$$

results as follows:—

Oxide of copper	31·76	31·60	6	31·58
Lime	21·36	21·82	6	21·41
Arsenic acid	30·68	32·41	2	30·57
Vanadic acid	1·78			
Phosphoric acid	8·81	...	1	9·47
Water	5·61	5·30	5	5·97

OCCURRENCE OF ARSENIC IN IRON PYRITES. BY M. BREITHAUPT.

[Poggendorff's *Annalen*, vol. lxxvii. p. 141.]

Breithaupt has examined a vast number of iron pyrites for arsenic, which metal he finds to be most extensively diffused, forming in general from ½—1 per cent. It is probable that all the iron pyrites upon heavy spar and fluor spar contain arsenic.

GLAUCOLITE. BY M. GIWARTOWSKI.

[*Journ. für Prakt. Chem.*, vol. xlvii. p. 380.]

This mineral is found at Baikal; it is imbedded in and penetrated by a greenish-yellow mica, has a blue colour varying from light to indigo blue, is transparent at the edges, has nearly the hardness of felspar, and a crystalline granular fracture. Its powder is white with a faint tint of lilac, which disappears on ignition; spec. grav. at 62° 2·65. According to the following analysis by Giwartowski, it has the formula RO, $SiO^3 + Al^2O^3$, SiO^3. For comparison, the former analyses by Bergmann and Jahn are added:—

	Giwartowski.	Bergmann.	Jahn.
Silica	50·494	50·583	25·50
Alumina	28·125	27·600	16·00
Lime	11·309	10·266	2·00
Magnesia	2·678	3·733	
Soda	3·103	2·966	
Potash	1·006	1·266	
Protoxide of manganese	0·595	0·866	0·50
Protoxide of iron	0·397	1·100	
Water	1·786	0·733	6·00
Loss	0·407	0·887	
	100·000	100·000	50·00

ARKANSITE AND BROOKITE. BY M. RAMMELSBERG.

[Poggendorff's *Annalen*, vol. lxxvii. p. 586.]

According to the researches of Rammelsberg, Arkansite is only a variety of Brookite. Arkansite was discovered by Powell at Magnet Cove, Hot Springs, County Arkansas, and described and named by Shephard. According to him, this mineral, when heated in a glass tube, furnishes neither moisture nor hydrofluoric acid; it is not changed before the blowpipe, and gives a dark yellow glass with borax. When boiled with sulphuric acid, it is converted into a yellowish-green mass, which

gives the reactions of titanic acid, whilst from the sulphuric solution (which gave a precipitate with sulphate of potash soluble in an excess of the precipitant) yttria was obtained, in which there might still possibly be zirconia and thorina. Arkansite fuses with bisulphate of potash, according to Shephard, to a yellowish mass, which, on ebullition with water, deposits a heavy white precipitate; he found 67 per cent. of titanic acid, and concluded from his experiments that the mineral was titanate of yttria.

Subsequently Shephard asserted the metallic acid to be niobic acid, and gave as his opinion that the mineral was niobate of yttria and thorina. He found the specific gravity to be 3·854.

Rammelsberg has recently examined this mineral; he found the spec. grav. to be 3·892, 3·923, 3·949. 1·7205 grm. lost on ignition 0·0045. After fusing the residue with bisulphate of soda, the mass readily dissolved in water at a gentle heat, the mineral consequently contains no tantalic, niobic or pelopic acid. The solution mixed with some sulphuric acid deposited on boiling 1·49 of a white powder, which proved to be pure titanic acid, both before the blowpipe as well as on being mixed with sugar and carbonized, and then heated in a current of chlorine. Ammonia precipitated from the filtered solution 0·217 of a white gelatinous substance, which also proved to be pure titanic acid on being tested in the same manner. By treating the precipitates with hydrofluoric acid, the mineral was found to be free from silica; on treating them with chlorine, there was no production of the difficultly volatile chlorides of yttrium, thorium, and zirconium.

On igniting a quantity of the separated titanic acid with carbonate of soda, 100 parts expelled 50·52 parts of carbonic acid. Now, since titanic acid contains, according to the experiments of Pierre, 38·86 per cent. of oxygen, and according to those of H. Rose 39·71, 1 atom of titanic acid expels 1 atom of carbonic acid. The error amounts only to 0·005 carbonic acid, which is trifling considering the small quantity employed.

In this treatment alone 94 per cent. of the weight of the Arkansite had been obtained of pure titanic acid; there remained 0·06 grm., in which nothing else could be detected with certainty.

Rammelsberg then observes, in opposition to an earlier statement of Breithaupt, that the crystalline form of Arkansite is identical with that of Brookite*, although the secondary

* The same conclusion had been previously arrived at by Prof. Miller of Cambridge, see Phil. Mag. for July 1849, p. 75.

faces in the two minerals differ. The specific gravity of the two minerals is—

Brookite.			Arkansite.	
4·125 to 4·169		Breithaupt.	3·854	Shephard.
3·810		Hermann.	3·952	Breithaupt.
4·128	transparent	H. Rose.	3·892	Rammelsberg.
4·131	crystals.	H. Rose.	3·923	Rammelsberg.
4·165	opake	H. Rose.	3·949	Rammelsberg.
4·167	crystals.	H. Rose.		
4·156		Rammelsberg.		

Further, the specific gravity of Arkansite is almost identical with that of Anatase; it may therefore be said that Arkansite is native titanic acid with the form of Brookite and the density of Anatase.

SMECTITE. BY L. A. JORDAN.

[Poggendorff's *Annalen*, vol. lxxvii. p. 592.]

This mineral, which occurs at Cilly in Lower Styria, and is likewise found at Zeny in Croatia, has been analysed by L. A. Jordan, who has found for it a composition corresponding to the formula $\left.\begin{matrix}Al^2O^3\\Fe^2O^3\end{matrix}\right\}3SiO^3+\left.\begin{matrix}MgO\\CaO\end{matrix}\right\}SiO^3+12HO$, viz.

Silica	51·21
Alumina	12·25
Peroxide of iron . .	2·07
Magnesia	4·89
Lime	2·13
Water	27·89

[To be continued.]

VI. *On the Multiple Sounds and Optical Phænomena produced by Vibrating Bodies. Theory of the Bow. By* M. J. Antoine*.

M. DUHAMEL has just published a novel and ingenious theory on the multiple sounds of sonorous bodies†. I know not what reasons have led him to imagine that justice has not been done him. On this point the learned academician is under an illusion. Physicists will render him justice, in spite of the disdain which M. Duhamel affects, when he supposes them as little advanced as at the period of Father Mersennus, and in spite of their well-founded belief

* From the *Annales de Chimie et de Physique* for October 1849.
† See Phil. Mag., vol. xxxiv. p. 415.

that they have long since possessed an exact, simple and direct theory of multiple sounds.

I regret that M. Duhamel has thought proper to be very brief in transcribing the state of science. It is with fear that I undertake to be his substitute in a delicate matter, which he has studied for many years; nevertheless, I shall venture to give an historical sketch, in order better to appreciate the present position of the question.

The phænomenon of multiple sounds has been known for a very long time, for Aristotle asks by what reason the grave sounds contain also sharper sounds. Mersennus however was the first physicist who submitted this to experimental analysis.

This ingenious observer classed the different higher sounds which accompany the fundamental note of a string, and found the same series for strings of different materials, tension and dimensions. The supplementary sounds which he could distinguish, placing himself in favourable circumstances, are the octave, the twelfth, the fifteenth, the major seventeenth, the nineteenth and the twenty-second. When accustomed to these experiments, it is easy to prolong the series beyond these limits. Mersennus classed also the tones which compose the multiple sounds of bells, as those, more curious still, which proceed sometimes simultaneously from the pipes of an organ.

In seeking the cause of this multiplicity of sounds, Mersennus made an experiment which might have led him to discover it; but the sagacity of his spirit was baffled, and the truth escaped him. He examined the vibrations of a long string, and seeing no division established, he concluded that strings do not divide to produce the simultaneous harmonics, and he therefore sought the explanation of the phænomenon in the wrong quarter.

The theory of multiple sounds did not make progress until experimental analysis had disclosed the cause of this multitude of sounds which may be separately drawn from one and the same sonorous body. It had been long known that a considerable series of successive sounds could be produced from the trumpet; Mersennus made the exact classification of them, and compared it to that of the different sounds which one pipe of an organ can give; but he could not discover the cause. The difficulty was great; and it was first solved, not upon aërial columns, but upon vibrating strings.

In 1673, William Noble and Thomas Pigot made that beautiful experiment, in which small pieces of paper, placed astride upon a string, are seen to remain immoveable in certain

positions, whilst beyond they are often projected from the string. Wallis considered the fact so curious and so novel, that he gave it a place in his Treatise on Algebra. It resulted clearly from the experiments of Noble and Pigot, that the same string can separately produce the different harmonics of the fundamental tone, and that, for this, it divides itself into aliquot parts which vibrate alternately in a contrary direction, remaining separated by immoveable points or nodes.

The cause of the phænomenon being thus clearly characterized in the particular case of vibrating strings, it remained to generalize it. Daniel Bernouilli by ingenious experiments treated the case, perhaps the most difficult, of wind-instruments, and finally experiment reached by degrees all possible cases, so that it is proved that a sonorous body can produce a considerable series of very different sounds, and that for each of them it divides into a corresponding system of vibrating parts and nodal surfaces.

It would perhaps be proper now to examine the connexion which exists between multiple sounds and the cause of the different sounds which bodies can give successively. However, to suit my purpose, I shall first say a few words on the processes which physicists have devised for drawing a varied succession of isolated sounds from the same sonorous body. I shall limit myself to the case of vibrating strings, the only one which has presented notable difficulties.

When Noble and Pigot made their capital experiment, only a single regular means was known of making a string produce its harmonics successively. It was not touched, it was not directly vibrated with the bow, but the harmonic which it was desired to raise was sounded at a distance, and instantly the string began to vibrate, to form its nodes and produce the harmonic. This communication of movement by the air and the supports of the string, or, as was then said, this communication of sounds by sympathy, had been discovered by Fracaster. Mersennus had made the experimental analysis of it with care, and had found that the sounds most apt to excite by sympathy the vibrations of a string form the different harmonics of the fundamental tone; he had also remarked that the fifth, the fourth, the major third and other consonances, produce also feeble vibrations, when the string is perfect, long, and stretched on a suitable instrument. The following case is perhaps not wholly unworthy to be mentioned. Two strings are stretched on a sonometer, and they are set so near to unison as to give beats when they vibrate simultaneously. Then if one of them be made to vibrate directly, the string which has not been touched experiences very

perceptible vibrations; and further, it causes beats to be heard. This experiment shows the imperfection of one of the processes which are indicated for tuning instruments.

In order to produce from a string the series of its harmonics in succession, Sauveur afterwards devised a second method, more convenient, and more effective than the first. As in the old process the string was not touched, the harmonic which it was desired to excite was sounded by the side, but he produced this first generating sound by vibrating the prolongation itself of the cord. The harmonics obtained in this manner are in some degree indefinite; the higher ones are not, it is true, so well characterized as the lower. Nevertheless, with a string one metre in length, Sauveur distinguished clearly thirty-two harmonics, and could hear more than a hundred and twenty-eight. Sauveur's method is, in this respect, much superior to the old process.

Sauveur did not seek whether it is possible to establish the subdivisions of the string, by vibrating it directly with the bow. This may however be attained, although the execution presents difficulties. If the bow is moved on the middle of the string, avoiding to impart the movement of totality which gives rise to the fundamental tone, we have at first only a harsh and very disagreeable grating sound. This fact, related by Wallis as known before him, is produced, as Wallis has remarked, when the bow is drawn on one of the points of the string which correspond to its division into aliquot parts, with this circumstance however, that the sound becomes less and less disagreeable, in proportion as the divisions are more numerous.

It must not however be imagined that the phænomenon mentioned by Wallis is constantly verified. For, on passing the bow on the middle of the string and modifying its velocity and its pressure, a sharp sound of great purity is in the end obtained, after some essays. When this sound is full, it is easy to sustain it as long as is desired, even whilst increasing considerably the pressure of the bow. By this means the uneven harmonics may be drawn from a string.

It is easy to distinguish in these experiments the formation of the nodes and their position. If the string gives, for example, the nineteenth harmonic, when observing it in broad daylight, it looks as if formed of nineteen equal spindles, placed one after another. The points where these spindles unite appear immoveable; if they are lightly touched with the finger, the sound which the bow maintains is not changed, and the string is not felt to tremble. If, on the contrary, one of the spindles is touched at a proper distance from their extremities,

the trembling of the string is felt and the sound is no longer obtained.

We may thus prove the existence of the nineteen vibrating parts, by passing the bow successively on each of them; by this manœuvre the sound will not be changed. If the bow approaches too near one of the nodes, the harmonic produced instantly disappears, and the fundamental generally supplies its place.

If it is desired to ascertain the position of the nodes, by means of light bodies placed astride upon the string, circles of paper must be used strung through the middle, or better still, a wire ring, in order that if the bow does not succeed at the first stroke in producing an harmonic, the light bodies may not be thrown off from the string.

The uneven divisions are not the only ones which may be produced by vibrating the string directly with the bow. The even divisions are obtained by drawing the bow skilfully at a proper distance from the nodes which it is wished to form. The success of the experiment will be rendered easy by touching an instant only one of the nodes.

The method which I have just pointed out may also serve to bring out with force, or to silence certain sounds in the concert of the harmonics which ordinarily accompany the fundamental tone.

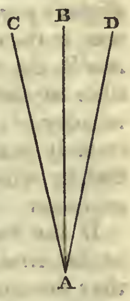
After this perhaps too long digression, I revert to the principal question. When the cause of the successive sounds which a body can give had been proved by experiment, the explanation of multiple sounds very naturally followed,—the explanation of which so greatly puzzled Father Mersennus. It is easy, in fact, to conceive that when different modes of vibrations, capable of existing isolatedly in a body, are produced in it together, each of them gives out the sound which corresponds to it. M. Biot has developed this theory with elegance; it has since been reproduced in the works of physicists. I know not for what reasons M. Duhamel has not even mentioned it.

The exactitude of the commonly admitted theory on multiple sounds, is confirmed and in some sort submitted to the eye, in the phænomenon of multiple strings and rods, which is perceived when they are made to yield different sounds simultaneously. To appreciate what there is completely demonstrative in this phænomenon, it is sufficient to bring attention to the facts.

When a rod AB, fig. 1, fixed in A, vibrates on each side its position of equilibrium AB between the extreme positions AC, AD, in a breadth which exceeds its thickness,

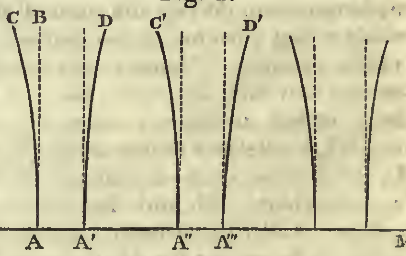
two images of the rod are seen perfectly traced and as it were immoveable in the positions AC, AD. The rod passes very rapidly each intermediate position, and reaches the extreme positions AC, AD, with velocities which are annulled and then increase again by imperceptible degrees. The images produced on the retina by the rod, when it is in the positions near the minima of velocity, where it remains almost immoveable for a comparatively long time, must be more vivid than in the other positions; thence the appearance observed. The two clearly visible images of the rod also approach by degrees, in proportion as the magnitude of the vibrations diminishes.

Fig. 1.



If the rod possesses at last a vibratory movement and a motion of translation, instead of two images, a very considerable number will be perceptible at regular distances over the whole extent that the movement of translation causes the rod to pass. Thus, if a knife be held in the middle, and one of its ends be struck on a fixed obstacle, leaving the knife free to rebound and vibrate, there will be perceived ten, twenty, thirty knives regularly distributed in front of the obstacle. If an object be struck with a bow, several images of the bow will be seen, each presenting a perfect and very distinct design of all the parts of the bow.

Fig. 2.

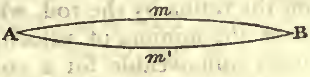


These multiple images are easily explained. Let us suppose that a rod AB, fig. 2, deviated towards AC, is left to itself, and that at the same time it is impressed by a movement of translation which carries the point A in the direction of the line AM. Let us take on AM the lengths AA′, A′A″, A″A‴...., traversed by the point A in the time which each simple oscillation takes to accomplish. When the point A is at A′, the rod will be at A′D′ at the end of its first simple oscillation; in the same way the rod will terminate its second, third .... oscillation in the positions A′C′, A‴D′... These positions are very near those which correspond to minima of velocity; the other positions are traversed very rapidly, and it is on this account that images of the rod are distinctly seen nearly in the positions AC, A′D, A″C′, A‴D′.... They will be seen simultaneously if the movement of translation is suitable.

Vibrating strings present analogous phænomena.

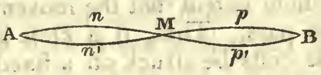
Let us suppose that the string vibrated produces the fundamental tone with energy, and gives the harmonics only in a slightly perceptible manner. Two very distinct images are then seen in the extreme positions AmB, $Am'B$, fig. 3; these images are visible at the very places where the velocity of the string is null or extremely small, in comparison with that by which it is affected when it traverses the intermediate positions.

Fig. 3.



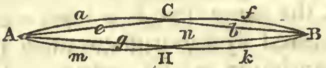
If the bow is drawn so as only to give the octave of the fundamental sound, two equal spindles are seen placed one after the other, and each one giving two images of the string in the positions where the velocity is annulled, as in AnM, $An'M$, MpB, $Mp'B$, fig. 4. These appearances are the immediate consequence of the division of the string into two distinct parts which vibrate separately.

Fig. 4.



If the fundamental tone be drawn from the string and at the same time its octave, so that these two sounds are very powerful in comparison with the other harmonics, which we will suppose to be very weak, four images of the string are then seen arranged as indicated in the figure. According to the explanation commonly admitted of multiple sounds, whilst the whole string vibrates, its two halves themselves vibrate, taking contrary curves. Hence it follows, that when the string quits the position AB, fig. 5, to take the direction toward the curve $AaCbB$, the first half of the string AC will present its concavity, for example, at AB, and the second half, CB, its convexity. During this transfer, the velocities of the different points of the string will tend to disappear successively: the curve $AaCbB$ is the spot which the different points of the string attain when their velocity becomes null.

Fig. 5.



After curving, as we have pointed out, the string will assume opposite curves, and will then return in the direction of the primitive curves, and so on. During these variations of form, the points of the string will pass from the position $AaCbB$ to the position $AeCfB$, where their velocity will be successively annulled, then to the position $AgHkB$, where the velocities are still null, then to the position $AmHnB$, where these velocities become null for the fourth time; and lastly, they will return to the position $AaCbB$, where the velocities were first

annulled, and where they will become null again successively for each point, if the decrease in amplitude of the vibrations is neglected.

From these considerations it follows, that during the complete vibration of the string, four distinct loci are formed in which the points of the string arrive successively without velocity, after having traversed the intermediate positions with a greater or less rapidity. Thence results the appearance of four distinct strings, curved as the lines of the figure indicate, and consequently the explanation of the optical phænomenon which is observed. It follows from these same considerations, that during an entire oscillation, the string strikes the air four different times, in passing from A*a*C*b*B to A*e*C*f*B, from this last position to A*g*H*k*B, then to the position A*m*H*n*B, and lastly, to the first position A*a*C*b*B. These four shocks are not identical: in the first and the third, the two halves of the string strike the air in a contrary direction, whilst in the second and the fourth, the string strikes the air in the same direction at all the points, nearly as if it moved without being divided. Thence it results that in the series of shocks which the tympanum of the ear receives, at each second shock the impression made on us must have something distinctive, and the periodical return of this phænomenon brings the simultaneous sensation of two sounds, the one the octave to the other. This is precisely the fact which observation indicates and which had to be explained.

All the preceding observations may be confirmed by completing in the following manner the experiment which we have just described.

If, by a suitable management of the bow, the octave of the fundamental tone is weakened, the spindles A*a*C*e*, C*b*B*f*, A*g*H*m*, H*k*B*n* will diminish in width, and will end in being imperceptible, when the octave has become very weak.

If, on the contrary, you weaken the fundamental sound, the spindles will persist, but the parts C and H will approach, will tend towards being confounded, and will in fact be so, when the fundamental sound is silenced.

The form of the curves will be easily determined upon which the different points of the string arrive successively without velocity, and the duration of the motion of each point in passing from one curve to the other, by aid of the formula

$$g=\alpha \sin \frac{\pi x}{l} \sin \frac{\pi a t}{l}+\beta \sin \frac{2 \pi x}{l} \sin \frac{2 \pi a t}{l},$$

which represents one of the possible movements of the string, and may specially represent that which we have just analysed.

In order to demonstrate the effects of the combination of two vibratory movements on a body, we have only to produce movements sufficiently slow to enable us to follow readily their different phases. We shall easily obtain this result by the aid of a long vibrating rod, by striking this rod in a suitable manner during its vibrations of totality, so as to produce a double vibratory movement. This experiment is not without interest.

I shall not pursue further the analysis of these different phænomena, nor shall I speak of the curious appearances presented by a vibrating string when left to itself. What I have developed suffices to render perceptible to the eyes the explanation of simultaneous harmonic sounds, as deduced from the fundamental experiments of Pigot, Noble and Sauveur.

Let us now examine the explanation of multiple sounds proposed by M. Duhamel. In this examination, we shall consider more specially vibrating strings, as offering the most elegant and interesting example of multiple sounds. To abridge and profit by the details on which we have already entered, we will suppose that the string gives simultaneously the fundamental tone and its octave.

In the first part of his memoir, M. Duhamel establishes this proposition: "When a body is made to vibrate by several causes which separately would produce the simple sounds which it can give, its surface generally divides itself into a certain finite number of parts, in each of which the vibrations have unequal durations. These different durations have relation to sounds corresponding to the different causes, and we are in the same position as if we had several separate surfaces, each having a particular movement of vibration."

In the vibrating string which gives the fundamental tone and its octave, the middle is the only point the vibrations of which differ in duration from those of the other points; I do not find here two finite portions of string in each of which the vibrations have unequal durations. Perhaps it will be said that one of the two finite portions is reduced to a point or to the contour of the middle of the string, but then the proposition would not be new with regard to strings. Still, however, this interpretation does not appear to me admissible, for, according to the proposition, we must be in the same case as if we had several separate surfaces, each having a particular movement of vibration; thence the fundamental tone would be solely attributable to the vibrations of the middle of the string, that is to say, of a single point; they would therefore be insensible with relation to the vibrations which give the octave. It is known that the fundamental tone may be very powerful, and

we have seen in the ordinary theory of sounds that the string produces this tone by striking the air at all its points.

M. Duhamel, whilst reproducing the preceding proposition in the same memoir, changes its sense very materially, when he says that each of the sounds exists in one or more finite parts of the surface, and appears to be perceptible there only. The isolation of each sound is therefore not absolute, but approximative; with this interpretation, the proposition of M. Duhamel allows of the string producing the fundamental tone by striking the surrounding air at all its points: only, the energy of the shocks will vary throughout the extent of the string, and may be very great in one finite part of the string, with reference to other parts. In this sense, the proposition would have little novelty.

In the second part of his memoir, M. Duhamel becomes more explicit; he abandons entirely the absolute sense of the first form of his proposition, that is to say, that which would be a real novelty, and admits, what is far from being anything new, that, during the multiple sounds of a body, there are portions of the surface which seem to give only one sound, although however we may be sure that they give out several others. In the case of the vibrating string, that is as much as to say, if I am not mistaken, that the middle of the string does not contribute directly to the production of the octave, and that the parts adjoining this middle contribute to it only in a scarcely perceptible manner, which is evident.

In the second part of his memoir, M. Duhamel adopts fully the received ideas on the multiple sound of bodies, but he presents them under a form which is peculiar to himself, and which we proceed to examine.

He admits, as every one does, that when a string produces the fundamental tone and its octave, its vibratory movement is composed of the two movements which correspond to the separate production of these two sounds; he does not seek to prove experimentally that it is so, but he shows that if the movement of the string is compounded as we have just said, there must result from it the simultaneous sensation of the fundamental sound and its octave.

In the common theory of multiple sounds, this consequence is in some sort immediate. When the entire string makes a vibration, its two halves which vibrate at the same time make two, and thence it follows that it strikes the air four times during a complete vibration; but the shocks are not identical, they only resemble one another alternately two to two; the ear is sensible to this periodical return of such shocks, and thence the origin of the compound sensation which is perceived.

M. Duhamel does not consider directly the shocks as they take place in reality, but he substitutes for them an equivalent system; instead of the string affected by the compound movement, he substitutes two identical strings placed close to each other, each executing simple vibrations which are an octave from each other. It is certain that the movement imparted to the air, whether by the single string, or by the two equivalent strings united, is very perceptibly the same, and our organ must be affected in the same manner in the two cases; now, it is known that two adjacent strings, one of which vibrates the octave of the other, cause the two sounds to be heard simultaneously; they must therefore be thus heard by virtue of the equivalent vibrations of the single string.

If I have rightly seized the point of view which M. Duhamel has taken, it appears from the preceding developments, that M. Duhamel, with all the physicists of the present day, finds the origin of multiple sounds in the manner in which the vibratory movement of bodies is composed. To establish this dependence, physicists consider directly the different shocks really imparted to the air, whilst M. Duhamel substitutes for the body vibrating with a compound movement, an equivalent system of simple movements, the impression of which on our organs is well known.

The common theory of multiple sounds is more direct than that of M. Duhamel, since in it we regard phænomena as they are really produced; it is also more elementary, and consequently more simple, as it does not require us to have recourse to propositions, one of which at least is not within the reach of every one. For these two reasons, the ordinary theory appears to us preferable in general; nevertheless, in certain complicated cases, we think that the mode of demonstration proposed by M. Duhamel may have the advantage.

Whichever of the two theories be adopted, one difficulty relative to the multiple sounds of bodies remains entirely to be solved. This difficulty arises from our not having as yet been able to submit to analysis the true theory of the bow.

The bow produces four principal effects. It sustains the equality of the sounds as long as is wished; it sustains also those feeble sounds which appear to die away under the fingers of the artist, whilst the sounds given by the strings, when pinched, are at first powerful, then soon become weakened, and are very quickly lost. The bow admits of giving to sounds different degrees of strength; it serves to impart to them particular qualities by appropriately awakening the harmonics; and lastly, it can bring out separately the different harmonics from a string. By these effects, the bow is the supreme master of sounds.

The bow acts thus by a series of slight impulses which it imparts to the string. These shocks, being continually renewed, keep up the movement which tends to become weaker; becoming slighter or more energetic, they change the amplitude of the vibrations, and consequently the strength of the sounds; by being applied to the different parts of the string, in suitable conditions, they are able to determine various modes of vibrations which give rise to the separate or simultaneous harmonics.

The impulses which the bow can impart are renewed with such great rapidity, that we cannot be assured of their existence when looking at the string. However, if the pressure of the bow is considerable and the velocity very moderate, the sounds produced present the same characters as if they were attributable to a series of shocks. Moreover a series of jerks may then be seen, which render manifest to the eye the different shocks of the bow. The experiment takes a new character of evidence when the tension of the string is very weak or the vibrations are slow.

When the experiment is performed which has just been indicated, and the jerks of the string are very perceptible, independent of the fundamental tone of the string, two supplementary sounds are heard which are the fundamental tones of the two portions of the string separated by the bow.

If the pressure of the bow be gradually diminished and its velocity increased, the jerks become more rapid, more difficult to observe, at the same time that the two supplementary sounds are weakened; as long as the supplementary sounds continue, they are as it were proofs of the shocks produced by the bow. Lastly, the jerks cease to be perceptible, and yet the two supplementary sounds are still heard, feebly it is true, but still perceptible enough to be distinguished, with a little attention. Thus the friction of the bow produces a succession of shocks, the origin of all the effects of this instrument.

It is not necessary to imagine that these shocks must be renewed in a regular manner in order to sustain the sound, for the sound of a string may be sustained by striking it gently with the finger and rapidly renewing the shocks almost in an arbitrary manner. The only difference that is remarked in the sounds sustained by the bow or by a rapid succession of shocks effected by pinching the string with the finger, is referable only to the delicacy and the lightness of the shocks of the bow.

The existence of the supplementary sounds, incontestable proofs of the shocks of the bow, is very clear and very decided, when the rapidity of the bow does not exceed certain limits. In the case where the bow moves in the ordinary way, the

supplementary sounds become so feeble, that the perception of these sounds might be attributed to a preconceived illusion of the mind, although analogy then lends its aid to the conclusions. Nevertheless, if any doubt still remained, it would be easy to remove it by the following observations.

When the bow causes a string to sound, its hairs execute transversal vibrations. To prove this, it suffices to pass a simple wire ring around the hairs: whilst the bow moves on the sonorous string, the wire ring, by its movements, renders the vibrations of the hairs evident.

The vibrations rendered perceptible by the wire ring are very remarkable when the bow is reduced to a single hair, and especially when the place of the hairs is supplied by a sonorous cord, rubbed with resin.

Ordinarily the tension given to the hairs of the bow is such, that the sound rendered by each hair, vibrating separately, is very grave with reference to the sound which the bow should draw from a string; whatever tension is given to the hairs of the bow or to the bodies which supply their place, the transversal vibrations are always established and are constantly rendered evident by the experiment, so that the shocks of the vibrating string against the hairs, and consequently the shocks of the hairs against the string, are incontestable.

It would be difficult to say *à priori* what influence the vibrations of the bow have on the clearness of the vibrations of the string, and consequently on the purity of the sounds produced. This influence is probably not to be neglected. It is known that a double-base, a tenor, or a violin bow cannot be employed indifferently for an instrument; it is also known that artists bestow great care in the choice which they make among bows of the same kind, and that they adopt certain tensions for the hairs. It would be interesting to submit the properties of bows to experimental analysis, and perhaps some result useful to practical music might be found. When the tension of the bow is such, that the sound of each hair has for octave of higher or lower order the sound of the vibrating string, or one of its consonances, would the sound produced be finer than under other conditions?

The analytical solution of the problem of vibrating strings is remarkably beautiful, and yet it is incomplete. To explain the effects produced, it is not enough, in practice, to recur, as is done in analysis, to very various primitive states. Ordinarily the initial state is the state of equilibrium under the influence of the tension of the string, and yet what variety of effects does the bow produce!

Ordinary analysis does not even suffice in the very simple

case in which the string is pinched. The initial figure and the primitive velocities are not arbitrarily given; they depend on the shock, and can be known only when the shock is exactly known.

The difficulty presented by the analytical theory of the bow, and even of the isolated shocks produced by pinching a string, is in fact that general and hitherto insurmountable difficulty, of subjecting the phænomenon of the shock to an exact analysis. It is known that only one very particular case of this phænomenon has been able to be approached, that only which Poisson has developed.

M. Duhamel does not consider the action of the bow under the point of view which has just been indicated. He regards the friction of the bow as equivalent, not to a series of shocks, but to a system of constant forces. A centre of attraction placed beside the string, far enough off for the displacements to be comparatively imperceptible, might therefore, according to this view, be a substitute for the bow. This seems at first sight scarcely probable. Be it as it may, if the new theory be exact, it should explain all the effects of the bow, and moreover its consequences should agree with the results of experiment.

M. Duhamel cites an experiment of verification, from which he has found that a circular bow, or rather a friction-wheel acting upon a stretched string, causes it to deviate from its position of equilibrium, and brings it in a very short time to a new position of equilibrium in which the friction maintains it, without the string continuing to produce a sound. This fact, the result of M. Duhamel's theory and experiments, may be regarded as negative with relation to this very positive fact, that hurdy-gurdy players sustain the sound of a string for whole hours, if desired. If M. Duhamel has obtained a negative result, this is caused probably by his not having placed the wheel in the conditions in which it acts in the manner of the bow.

In fine, it is not easy to see how M. Duhamel's theory accounts for the four principal effects of the bow, and how it has reference to the vibrations which the hairs of the bow constantly execute when they are passed over a sonorous string.

VII. *Notices respecting New Books.*

Relation des Expériences pour déterminer les principales lois physiques, et les données numériques qui entrent dans le Calcul des Machines à Vapeur. By M. REGNAULT*. 1 vol. of 800 pp. 4to, *with a folio volume of plates.*

SCIENCE, like literature and the arts, has now and then its works of luxury. We have a proof of it in the publication which is the subject of this article; it is indeed one of the most splendid that has ever enriched science. We shall not pause at the beauty of the typographic execution, although the perfection of the plates, the great scale on which they are done, are rarely met with in works of this kind. But here the luxury lies in the very substance of the work, in the number, the precision and the details of the experiments, in the variety and perfection of the apparatus, and in the richness of the results.

The aim with which M. Regnault began his researches, was the determination of the principal physical laws and the numerical data, which enter into the calculation of steam-engines. Hitherto the theoretical calculation of these machines has been constantly based upon laws which could only be considered as hypotheses; but mechanicians have long asked for a general work which should establish these fundamental laws upon a series of direct experiments, executed with the means of precision which the physical sciences now offer us. The French government having conceived the happy and noble idea of executing this work, placed the necessary funds at the disposal of M. Regnault; this distinguished physicist has profited by it to raise to science, one of those monuments which are durable in proportion as the foundation on which they rest is more solid, and the style is more severe and simple. That is to say, that M. Regnault determined to penetrate to the bottom of his subject, that he has not allowed himself to be carried away by any hypothesis, and that he has shown himself in physics, what Saussure did in geology, one of those philosophers whose observations will remain, whatever may be the phases through which science is destined to pass.

While rendering full and entire homage to the qualities which constitute M. Regnault one of the first physicists of this æra, we are far from questioning the merit of those who, following another course, advance science in a manner different indeed, but who leave behind them traces no less deep. There are men whose creative imaginations, opening out a totally new path, march to the conquest of unexpected truth, with a good fortune and success which are a consequence of the divination that produces a perfect appreciation of the laws of nature, joined to the inspiration of genius. The physicists of this school, if the name of school can be applied to that which is not a method, but a gift of nature, differ essentially from

* This interesting review and summary of the beautiful researches of M. Regnault we have transferred from the pages of the *Bibliothèque Universelle de Genève*; it is from the pen of M. de la Rive.

those who, like M. Regnault, follow a route necessarily leading to the end and leaving nothing to chance. The latter improve, rectify, what the others have discovered; they often complete it; sometimes they diminish its importance; in all cases they co-ordinate the different parts of it, in a manner to render evident the true capacity and just proportions of the new elements with which science is enriched. Without the first, no science; without the second, no true science. But we shall not further attempt to compare that which, in reality, is not susceptible of it, since the elements of the comparison are so different. Let us confine ourselves to a summary of our idea, in declaring that, if Regnault is the type of one of the schools, Faraday is the glory of the other; this is to state in two words all that science owes to them, and the services she still expects from them.

Returning to our subject. The question is the theoretical calculation of the work done by steam-engines, which requires, according to M. Regnault, a knowledge of the following laws and data:—

1. The law which connects the temperatures and the elastic forces of aqueous vapour in a state of saturation.

2. The quantities of heat which 1 kilogramme of liquid water at 0° Cent. must absorb, to be reduced to vapour in a state of saturation under different pressures.

3. The quantities of heat which 1 kilogramme of liquid water at 0° Cent. must absorb to elevate its temperature to that at which it becomes vaporized under different pressures.

4. The specific heat of steam in different states of density and at different temperatures.

5. The law according to which the density of the steam in a state of saturation varies under different pressures.

6. The coefficients of the dilatation of steam acquired in its different states of density.

But before entering upon the investigation of these laws, M. Regnault was compelled to undertake long preliminary researches, in order to determine a great number of auxiliary data, which appeared fixed with certainty by the earlier labours of other physicists, which however they were not, as experience has shown. These researches are in particular relative to the laws of dilatation and compressibility of elastic fluids, and to the measurement of temperatures. The first seven memoirs contained in the volume before us, are essentially devoted to these. It is only in the three last, the eighth, ninth and tenth, that M. Regnault begins to attack the very questions, the solution of which was the primitive intention of his great task, in treating the three first points denoted above.

We reserve the analysis of these last three memoirs for another article. At present we shall confine ourselves to the examination of the preliminary researches. We do not pretend at all, be it understood, to give a complete account of them. Of seven memoirs which are devoted to them, there are three which we shall be content merely to mention, namely, that which relates to the determination of the weight of the litre of air and of the density of mercury,

that of which the object is the absolute dilatation of mercury, and lastly, the memoir on the compressibility of liquids, and in particular of mercury. This last, which contains a detailed explanation of the methods to be employed, does not include a great number of decisive experiments, the author not having had time to execute researches so considerable as the complete study of the subject would have required. One of his pupils, M. Grassi, has lately made known the results he obtained by following the methods indicated by M. Regnault, and by using his apparatus. As to the other two memoirs, they contain important numerical data, it is true, but the interest of these to us, lies essentially in the use which the author makes of them in the more general researches of which we are about to treat. Nevertheless the admirable exactitude of the methods applied by M. Regnault, and the remarkable precision with which he operates, lead us to report here some numerical data, the importance of which will be appreciated by all philosophers, namely, the weight at Paris, that is in the latitude 48° 50′ 16″, of the litre of air, nitrogen, oxygen, hydrogen, and carbonic acid, at the temperature of 0° Cent. and under the pressure of 0·760 millim., as well as the density of mercury at 0°.

	grm.
Atmospheric air......	1·293187
Nitrogen gas........	1·256167
Oxygen gas	1·429802
Hydrogen	0·089578
Carbonic acid........	1·977414

A litre of mercury at 0° C. weighs 13595·93 grms., and a litre of water at the maximum of density 1000·00 grms. Thus the density of mercury is 13·59593; the density of mercury in relation to air, which it is sometimes requisite to know, for instance in the measurement of heights by the barometer, is 10513·5 at the temperature of 0° and under the pressure of 0·760 millim. at Paris.

The two essential portions of the preliminary researches of M. Regnault are, as we have said, that relative to the laws of dilatation and compressibility of elastic fluids, and that of which the object was the measurement of temperatures. We shall run over them successively, indicating the most striking points.

Researches relative to the Laws of Dilatation and Compressibility of Elastic Fluids.

M. Gay-Lussac, in the first years of this century, at the close of a great number of experiments, came to the determination that the coefficient of dilatation between 0° and 100° C., was the same for all gases, and for vapours when they were at some distance from their point of condensation, and that its value was 0·375. This law and this coefficient, subsequently verified by Dulong and Petit on hydrogen and atmospheric air between much more distant limits of temperature, were admitted by all physicists and employed in all calculations. Combined with the law equally admitted under the

name of Mariotte's law, that *the densities of air at equal temperature are proportional to the pressures*, the law of Gay-Lussac furnished to geometers, and in particular to Laplace, the means of arriving by calculation at simple and general notions on the constitution of elastic fluids. It was believed that thus was found, in the gaseous state, the state of matter most fitted to bring to light the general qualities of bodies, since it was independent of the special nature of each of them.

Already, it must be remembered, the researches of Dulong and Petit, on the cooling of bodies in different gases, had revealed a special influence independent of the density and the other general conditions of elastic fluids, and proper to each in particular. After the experiments of Faraday on the liquefaction of gases, several physicists, and particularly MM. Œrsted and Svendsen in 1826, and M. Despretz in 1827, perceived that several gases compared with atmospheric air, departed from the law of Mariotte, and presented compressibilities increasing with the pressure, even from the point of two atmospheres. Some uncertainty was equally manifested respecting Mariotte's law, applied to atmospheric air, but they appeared to vanish completely after the excellent researches of MM. Arago and Dulong, executed in 1827 at the request of the Academy of Sciences of Paris, researches which caused the law of the compressibility of atmospheric air to be regarded as directly verified up to twenty-seven atmospheres, and capable of being extended without any notable error much beyond this limit. M. Pouillet announced lately that he had ascertained that oxygen, nitrogen, hydrogen, binoxide of nitrogen, and carbonic oxide follow, up to 100 atmospheres, the same law of compression as atmospheric air. We shall see that these results, and that of MM. Arago and Dulong, must only be admitted with some reservation, and that M. Regnault has demonstrated by direct methods that they are not rigorously exact, after having already remarked that they were not readily reconcileable with his experiments on the dilatation of gases. We will therefore in the first place say a few words on this subject.

The coefficient of Gay-Lussac had been generally admitted for a great number of years, when a Swedish physicist, M. Rudberg, threw doubts on its exactitude, showing by a series of experiments carefully made, that it was too high, and that its true value should be comprehended between 0·364 and 0·365.

M. Regnault, considering that new experiments were necessary to remove all the doubts in this respect, gave himself up ardently to them, at the time when M. Magnus was making analogous researches in Berlin.

Five series of experiments were made on atmospheric air; in the four first, which only differed from each other in the methods employed, and were based on the same principle, the dilatation of the gas was determined in an indirect manner. Direct measurement was made of the increase of elastic force which the gas, brought into a volume sensibly constant, received through the elevation of the temperature, and the dilatation was concluded, by making use of

Mariotte's law. The fifth series of experiments was for the purpose of seeking the dilatation of the gas in a direct manner; for this it would be requisite to enclose it in a very elastic envelope, so that it might dilate freely without change of the elastic force, and that the increase of volume it would undergo might be measured with precision, the whole mass remaining at the same temperature. It was difficult to realize these conditions practically, but M. Regnault has approached them, by following a method analogous to that already employed by M. Pouillet for his air pyrometer, and which consists in having a kind of reservoir filled with mercury, communicating with the balloon containing the gas experimented on, and causing the mercury necessary to give place to the gas, to flow in such a manner that it may obey its dilatation, without change of elastic force.

It is difficult to conceive all the precautions which are required both in this and the other methods, to ascertain exactly the capacity of the balloons and tubes which contain the gas, to ensure thorough desiccation of the air, to bring to one given temperature all the parts of the apparatus, and in particular the columns of mercury, which, by their height, serve to measure the variations of the elastic force, or those of the volume, and to determine with the necessary precision the heights of the columns of mercury. The very clear detail of his experiments must be read in M. Regnault's work itself, the description of all his apparatus must be followed on the plates which accompany his memoirs, to understand the nature of the practical difficulties which are presented by researches of this kind, and the manner of securing himself from all the causes of error, so numerous, which the experimenter necessarily meets on his way. One of the most difficult operations, until the habit has been acquired, is the measurement of the height of the mercury raised. M. Regnault employed for this purpose the cathetometer of Gambey, which, by its vernier, gives an immediate reading to the fiftieth of a millimetre. This cathetometer is a divided vertical scale, fixed on a solid point, and bearing a traveller furnished with a perfectly horizontal lens. The operator looks with the lens toward the level of the mercury raised in the tube, then the lens is lowered, and he looks at the upper end of a screw, the lower end of which just touches the surface of the mercury in the vessel in which the tube is plunged; by adding to the difference of level thus obtained, the distance between the two points of the screw which had been previously measured with the same instrument, the total height of the mercury raised is obtained.

The five series of experiments gave the following means for the volume of gas at 100°, its volume at 0° being 1 :—

First series..........	1·36623
Second series........	1·36633
Third series	1·36679
Fourth series........	1·36650
Fifth series	1·36706

The first four methods, although different, give nearly the same

coefficient; they are not of equally easy execution, and perhaps are not susceptible of the same degree of exactitude; nevertheless they lead to the same result, and this is what is required for the determination of numerical elements with some degree of certainty. In these first four series of experiments, the coefficient of dilatation is deduced, by the calculation of the change of elastic force which a given volume of gas undergoes when its temperature varies from 0° to 100°, and the mean of the very closely approximate results gives

0·003665.

The fifth series furnishes the number 0·003670, which differs notably from those given by the other four; which depends, as subsequent researches showed, on the fact that in these last experiments the gas dilated freely, preserving the same elastic force.

After having determined the coefficient of the dilatation of air by different methods, and having thus studied these methods themselves, and ascertained that when properly executed they conducted to the same result, M. Regnault selected for operation upon the other gases, that which had appeared to him most susceptible of exactitude, and of surest and easiest execution; this was the method employed in the second series of experiments; he also occasionally made use of that of the fourth; finally, he operated with that of the fifth, to obtain comparatively the dilatation under *constant pressure* and under *constant volume.* He did not succeed in obtaining results with oxygen, not being able, in spite of every precaution, to prevent the action of this gas upon the mercury.

The following are the results which demonstrate, that for all the gases, except *hydrogen*, the coefficient of dilatation, determined by direct observation of the increase of volume, which the same mass of gas undergoes from 0° to 100°, its elastic force remaining constant, is greater than that which is deduced by calculation from the observation of the elastic forces presented by an identical volume of gas at 0° and 100°.

Mean Coefficient of Dilatation between 0° *and* 100° *Centig.*

	Under constant volume.	Under constant pressure.
Hydrogen	0·3667	0·3661
Atmospheric air	0·3665	0·3670
Nitrogen	0·3668	„
Carbonic oxide	0·3667	0·3669
Carbonic acid	0·3688	0·3710
Protoxide of nitrogen	0·3676	0·3719
Sulphurous acid	0·3845	0·3903
Cyanogen	0·3829	0·3877

This table shows equally that the different gases present coefficients of dilatation differing very notably from each other, contrary to what has been long believed. No one will overlook the fact that the highest coefficients of dilatation are presented by those which are most easily liquefiable.

It remained to determine the influence exercised on the coefficient of dilatation by the pressure to which the gas is subject. M. Regnault studied this influence on various gases, by subjecting them to both a less and a greater pressure than that of the atmosphere. The apparatus employed for researches of this kind were the same as for the preceding; the balloon which contained the gas was successively surrounded with boiling water and melting ice. The pressure was measured in the cases both of a high and low pressure, by a column of mercury, the level of which was given by the cathetometer. In the first case, as the pressure was carried up to four atmospheres, the column was sometimes very long, and much care was required to measure it very accurately. It was requisite also, among other precautions, to protect the column of mercury from the heat coming by radiation from the source which maintained the balloon at the temperature of boiling water, and to take account by direct appreciation, of the increase of volume which the balloon acquired from the effect of a high pressure, which was done by filling it with water instead of gas.

The experiments showed that, setting out from the atmospheric pressure, the coefficient of dilatation of air diminishes and increases with the pressure in a very marked manner. Thus, under a pressure of 109·72 millims. at 0°, and of 149·31 millims. at 100°, it is 1·36482, and under a pressure of 3655·56 millims., at 0°, and 4992·09 millims. at 100°, it is 1·37091. The densities of gas at the temperature of melting ice, varied between the same limits of pressure, that is to say, from 109·72 millims. to 3655·56 millims., from 0·1444 up to 4·8100 (that of air at 0° under the pressure of 760 millims. being=1), that is to say, in the proportion of 1 to 33. Hence it results, therefore, that the law admitted by physicists, that air dilates by the same fraction of its volume at 0°, whatever may be its density, is inexact, and that, on the contrary, it dilates between the same limits of temperature, by quantities which are the more considerable in proportion as the density of the gas is greater, or in other terms, as its molecules are more closely approximated.

The same is the case with carbonic acid, the dilatation of which increases progressively with the pressure, much more rapidly than that of atmospheric air.

Other experiments, made equally under different pressures, but executed by the method of constant pressure, led to the same result, namely, an increase of the coefficient of dilatation growing with the pressure. The increase is even more rapid when this method is used, than when the coefficient of dilatation is determined by means of the variation of the elastic forces. Carbonic acid, above all sulphurous acid, present a dilatation increasing very rapidly with the density; thus for a change of pressure so slight as that from 760 millims. to 980 millims., the coefficient of dilatation of sulphurous acid changed from 0·3902 to 0·3960; and nevertheless, the gas under a pressure of 980 millims. is not yet near to the 0° of its point of liquefaction.

A somewhat remarkable thing is, that the difference between the

dilatations of atmospheric air and carbonic acid gas is much more considerable when the pressure is the same at 0° and at 100°, than in those in which the dilatations are calculated according to the changes of elastic force.

Hydrogen is the only gas of which the coefficient does not alter with the pressure, at least up to that of four atmospheres, while that of air continually increases from the point of ordinary barometric pressure, under which the two gases have sensibly the same dilatation.

In general, the more considerable the pressure under which the gases are examined, the greater difference is found between their coefficients of dilatation. Inversely, the coefficients of dilatation of the different gases approach to an equality in proportion as their pressures become weaker, so that the law which says that gases have all the same coefficient of dilatation, may be considered as a limited law, which applies only to gases in an extreme condition of dilatation, but which departs from the reality in proportion as the gases are more compressed, in other terms, as their atoms are more closely approximated.

The first task of M. Regnault, which we have just rapidly surveyed, confirms us in that opinion which we announced at the outset, namely, that a great error is committed in supposing the gaseous condition to be a state in which the physical properties of the body, with the exception of the density, are independent of its peculiar nature, and connected solely with its general constitution. We see, in fact, that the coefficient of dilatation of the gas varies with its nature, with its density, and with the mode in which it is operated on. Hydrogen alone appears to present more general relations in this respect.

The direct determination of the densities which forms the object of M. Regnault's second memoir, conducts to consequences in every respect analogous. This determination, effected by a method much more simple than those hitherto employed, and at the same time very exact, gives the densities at 0° and 100° of the different gases in proportion to air, which allows of the deduction from it of the coefficients of dilatation by the method of weighing. The values of these coefficients are nearly the same as those which had been obtained by the direct methods.

The densities at 0° and under the pressure of 760 millims., found by M. Regnault for nitrogen, hydrogen, oxygen and carbonic acid, differ very little from those to which MM. Dumas and Boussingault arrived; they are each the mean of very slightly differing numbers, furnished by a great number of experiments. But if they are taken at other temperatures, and under other pressures, the results are no longer the same, and they furnish curious consequences. Thus by taking the density of carbonic acid at 0° under different pressures, but all lower than the atmospheric pressure, it is ascertained that this gas departs from Mariotte's law in a very marked manner, whilst at the temperature of 100° it sensibly follows this law. It is evident, therefore, that according to the temperature and pressure

at which the operation is carried on, the density of carbonic acid bears a different proportion to that of atmospheric air, although in each case, as it is scarcely necessary to mention, care has been taken to bring the air to the same temperature and the same pressure. The following are a few figures in support of this conclusion :—

The density of carbonic acid gas at 0° is—

Under the pressure of	760·00 millims.	1·52910
..	374·13 ..	1·52366
..	224·17 ..	1·52145

The density of the same gas at 100° is—

Under the pressure of	760·00 millims.	1·52418
..	383·39 ..	1·52410

Hence it results, that the atomic weight of carbon, deduced from the density of carbonic acid gas, varies according to that of the densities selected. Thus the atomic weight of 75·000 admitted by M. Dumas, conducts to the theoretical density of 1·52024 for carbonic acid, a density very near to that found for this gas at 0°, and under the pressure of 224·17 millims., while the density of 1·52910 ascertained for carbonic acid at 0° and under a pressure of 760 millims., conducts for the atomic weight of carbon to 76·60, a number closely approximate to that of 76·44, for a long time adopted by chemists on the authority of Berzelius. This example shows the uncertainty which there is a risk of meeting in the determination of atomic weights deduced from the density of elastic fluids.

The first researches of M. Regnault showed, then, that the law of equality and uniformity of the dilatation of elastic fluids was not general, and that Mariotte's law seemed to be equally defective; as we have already mentioned, other physicists had arrived at the same result in regard to this last law, at least for gases capable of being liquefied by high pressure. In his sixth memoir M. Regnault attacks the question directly, and after a critical analysis of earlier labours, passes to the description of his own experiments. They were made by means of apparatus erected at the College of France, in a special building, which had been constructed, at the request of M. Savart, for hydraulic experiments, the execution of which was prevented by the premature death of that skilful physicist. This building consists of a square tower of two stories, the total height being 12·5 metres. A mercurial manometer is fixed in the inside against a vertical wall, which is of the entire height of the building. A cast-iron vessel, closed by a cylindrical reservoir, which serves as the reservoir of mercury, is fitted with a cylindrical adapter bearing the necessary tubulures, three of which are vertical; a little hydraulic force-pump serves to compress the air in the reservoir, with which it communicates by a horizontal tubulure. For the experiments on the compressibility of gases, the author used only two of the vertical tubulures; the third was hermetically closed. He placed in them two glass tubes, one of which, three metres high, communicated with the apparatus which contained the air to be compressed, and the

other, of considerable height, was intended to contain columns of mercury making equilibrium at pressures carried as high as thirty atmospheres. Nothing can be more interesting to read, to persons who have to perform the manipulations necessary in experimental researches, than the details of the ingenious and simple means by which M. Regnault succeeded in cementing into the tubulures and connecting end to end, the different tubes of the apparatus.

The plan by which he contrived to effect this union, is of such convenient and sure application, that its employment will doubtless become general, especially on account of the facility with which it allows of the instantaneous mounting and dismounting of the different parts of the manometer, without risk of accident. We cannot, to our great regret, dwell upon these details, which would lead us too far, any more than upon the precautions necessary for the exact measurement,* with the cathetometer, of the difference of level between the surface of the column of mercury which rises in the little tube containing the compressed air, and the surface of the column of mercury raised in the large open tube, communicating of course with the atmosphere. One point of some importance, is to take into account the difference of height, and consequently of the atmospheric pressure, due to the elevation of the second of the mercurial surfaces above the level of the mercury in the reservoir; M. Regnault found, by direct observation, that this element of correction could not be neglected any more than that relating to the increase of the capacity of the tubes under the influence of the high pressures to which they were subjected. Finally, it is necessary to wait, before making an observation, till the heat liberated in the gas in consequence of the compression has been dissipated, and to employ a current of water to maintain it, during the experiment, at a temperature as constant as possible.

The gases on which M. Regnault's experiments were made, are atmospheric air, nitrogen, carbonic acid and hydrogen; there were several different series of experiments made on each gas, from the ordinary atmospheric pressure up to the pressure of nearly thirty atmospheres, expressed by columns of mercury of about twenty-two metres in height.

The detailed tables of the experiments contain all the numerical data which permit the calculation of the relation $\frac{\left(\frac{V_0}{V_1}\right)}{\left(\frac{P_1}{P_0}\right)}$, a relation which would be equal to 1, if Mariotte's law, that *the volumes are in inverse proportion to the pressures*, were correct. Now it is seen that atmospheric air does not follow this law strictly, and that it really becomes a little more compressed than it should according to that law. It might be feared perhaps that the difficulty of exactly appreciating the volumes V_0 and V_1 was the cause of the result contrary to Mariotte's law; but supposing that this circumstance gave rise to an error in the case of a certain pressure, the error ought to be always the same for all pressures.

Now the numbers which express the relation $\frac{\left(\frac{V_0}{V_1}\right)}{\left(\frac{P_1}{P_0}\right)}$ increase regularly with the pressure. Thus, for instance, the difference between the elastic force observed and that deduced from Mariotte's law rises to 114 millims. under the pressure of 9336·41 millims., and of 18548·98 millims.

Nitrogen gas in like manner becomes more compressed than it should according to Mariotte's law; and the relation $\frac{\left(\frac{V_0}{V_1}\right)}{\left(\frac{P_1}{P_0}\right)}$ augments regularly with the pressure; it increases, however, less rapidly than with atmospheric air. It is therefore very probable that oxygen gas would depart still further from Mariotte's law than common air and nitrogen.

Carbonic acid gas departs very notably from Mariotte's law, even from the point of a single atmosphere of pressure; the relation $\frac{\left(\frac{V_0}{V_1}\right)}{\left(\frac{P_1}{P_0}\right)}$ which is, for $\frac{V_1}{V_0}$ equal to 2, from 1·0076 when the initial pressure is a single atmosphere, becomes 1·0999 when the initial pressure is 12·66 atmospheres. There are even still greater aberrations: thus, when the volume 1 of carbonic acid gas, having an elastic force of 6820 millims., is reduced to the volume $\frac{1}{3{\cdot}50}$, its elastic force becomes 20284 millims., and the relation $\frac{\left(\frac{V_0}{V_1}\right)}{\left(\frac{P_1}{P_0}\right)}$ acquires a value of 1·1772.

Mariotte's law therefore cannot be regarded even as an approximative law for carbonic acid gas, when this gas is observed under rather high pressures.

Hydrogen does not follow Mariotte's law better than the other gases, but what is very remarkable, it departs from it in the contrary direction. Its compressibility, far from increasing, as in the rest, diminishes in proportion as the pressure augments. The elastic force of this gas is therefore analogous to that of a metallic spring, which offers a greater resistance in proportion as it is subjected to a greater compression. The relation $\frac{\left(\frac{V_1}{V_0}\right)}{\left(\frac{P_0}{P_1}\right)}$ is for hydrogen always sensibly less than 1, but greater than 0·99000 within the limits of M. Regnault's experiments.

Hence it results that Mariotte's law must not be regarded as a mathematical expression of *the perfect gaseous condition*, for then *hydrogen* gas would constitute a *more than perfect elastic fluid*.

The elastic resistance which hydrogen presents, probably would not increase indefinitely with the condensation; a certain state of condensation should exist in the vicinity of which Mariotte's law would be followed quite strictly, then, the condensation increasing, the hydrogen gas would depart again from the law, but in a direction contrary to its original aberrations, and the relation $\frac{\left(\frac{V_0}{V_1}\right)}{\left(\frac{P_1}{P_0}\right)}$ would become greater than unity, and would continue to increase up to the moment of the liquefaction of the gas.

The temperature necessarily exercises a great influence upon this phænomenon, for carbonic acid gas, which at 0° departs from Mariotte's law, even under pressures less than that of the atmosphere, no longer departs from it at the temperature of 100°. Atmospheric air itself departs from it much less between the same limits of density, at elevated temperatures, than at the ordinary temperature. It is probable that a temperature might easily be obtained, at which the divergences would become insensible, and at a temperature still more elevated atmospheric air would again depart from Mariotte's law, but in the contrary direction, that is, in the direction in which hydrogen gas departs from it at ordinary temperatures. Similar circumstances, but in an inverse order, would, it is not unlikely, present themselves in hydrogen gas, if it were submitted to lower or higher temperatures.

It is probable then that Mariotte's law is a law which holds for each gas in a certain state of density and at a definite temperature. We have then $\frac{\left(\frac{V_0}{V_1}\right)}{\left(\frac{P_1}{P_0}\right)}=1$. The state of condensation remaining the same and the temperature decreasing, the compressibility becomes greater than that which would result from the law, and the relation is >1. The temperature rising, the gas always taken in the same state of condensation presents a lower degree of compressibility than that which is deduced from the law; the relation is <1. Thus, the temperature at which the relation $\frac{\left(\frac{V_0}{V_1}\right)}{\left(\frac{V_1}{V_0}\right)}$ becomes less than 1, after having been greater, varies necessarily with the density for each gas, and is the more elevated in proportion as the density is more considerable.

It would be very interesting therefore to study the compressibility of gases at elevated temperatures, a study very difficult and almost

impossible, on account of the necessity of stationary elevated temperatures. However, experiments of this kind might be made with success up to the temperature of boiling water, determining by means of the balance the weight of gas which fills the balloon under different pressures, and at the fixed temperatures of melting ice and boiling water. M. Regnault describes the apparatus, composed essentially of two copper globes, which he destines to experiments of this kind; but the experiments themselves have not yet been executed. M. Regnault attaches the more importance to them, that they not only allow of the determination of the influence of that important element, the temperature, but that they may moreover serve to control the method followed in the other experiments, to which the objection might be made, that the anomalies perceived in atmospheric air are due, at least in part, to a condensing action of the surface of the glass tube which contains the air.

To sum up the foregoing, we see that the very complete and precise researches of M. Regnault conduct to the conclusion, that neither the law of Gay-Lussac on the dilatation of elastic fluids by heat, nor that of Mariotte on the constant relation which exists between their elastic force and their volume, are laws of nature, but that they are only approximative, and consequently only true between certain limits. The aberrations manifest themselves for both the laws in tolerably similar circumstances, and demonstrate the intimate relation which exists between the two classes of phænomena. Thus the coefficients of dilatation are greater in proportion as the density of the gas is more considerable; the same holds of the compressibility, hydrogen being the sole exception in both cases. Carbonic acid gas dilates more, and likewise experiences a greater compression, than atmospheric air in the same circumstances. Many other parallels of the same kind might be made, but I prefer confining myself, in concluding this account, to dwelling for a moment on the consequences which, it appears to me, may be deduced from M. Regnault's researches, in reference to the constitution itself of elastic fluids.

It has long been said that in gaseous substances the influence of their special nature is no longer perceptible, and that their physical properties, excepting the density, depend alone on an agent common to all, namely heat, and that it is for this reason that the laws they present are general and the same for all, such, in particular, as Gay-Lussac's and Mariotte's laws. Error in the consequences, therefore error in the point of departure.

But what M. Regnault's labours appear to prove, is, that the less the density of the gases, the more general and similar appear the laws they present. Only it is necessary, in the case of those of which the low density does not result, as in hydrogen, from their special nature, that an elevation of temperature should compensate, as far as possible, for the loss of elastic force to which the diminution of density subjects them. In one word, what is required is, that the atoms should be separated as widely as possible, and the elastic

forces be at the same time as great as possible; conditions which are opposed to each other, except by bringing the temperature into action. Then the special nature of the atoms no longer interposes in the phænomena, and the laws exhibit a generality which they did not possess when, the atoms being more closely approximated, their mutual action influenced the result. Thus hydrogen presents the same coefficient of dilatation under different pressures, and the other gases come nearer to having the same coefficient in proportion as their density is less. With regard to that aberration from Mariotte's law, in a contrary direction from other gases, presented by hydrogen, and which the other gases would probably present at high temperatures, it would be a general consequence, and therefore similar for all elastic fluids, of the laws which regulate the constituent elastic force of the gas; it would simply prove that the intensity of this force is not so simple a function as has been supposed, of the distance which separates the atoms.

Finally, we will remark, that these considerations, based on the experimental researches of M. Regnault, are very contrary to the opinion long received, that *under the same pressure and at the same temperature, equal volumes of all gases contain the same number of atoms.* They are of a nature, on the other hand, to lead to the presumption, that the densities of gases do not at all express the relations which exist between their atomic weights; conclusions, moreover, which purely chemical considerations tend to corroborate, and at which I had already arrived in 1833, when drawing up a report on the *Traité de Chimie* of Berzelius.

The alteration produced in the chemical properties of gases by their less or greater degree of density, is another proof to be added to those I have already indicated, that elastic fluids have an individual molecular constitution; a kind of cohesion, as it were, when their atoms are not removed beyond a certain limited distance from each other. This interesting and delicate subject, which I have but just touched upon, can only be treated properly when new facts shall have been added to those which the sagacity and perseverance of physicists like M. Regnault have enriched it. I therefore terminate here the report of this portion of the researches of the French philosopher, and now enter upon the memoir in which he treats of the *measurement of temperatures.*

The Measurement of Temperatures.

This subject, which seemed to have been exhausted by MM. Dulong and Petit, in their excellent investigations made thirty years ago, has been wholly worked over again by M. Regnault, this revision having been rendered necessary from the circumstance that, by his own experiments, the French physicist had so importantly modified the data on the dilatation and compressibility of gases previously received.

The problem of the measurement of temperatures is perhaps the most difficult of solution that physics present. We have, indeed, no direct means of measuring the quantities of heat absorbed

by any body in given circumstances. This absorption of heat is only to be perceived by the changes which take place in the state of the body or by its dilatation. Now when the dilatations which different bodies undergo under identical circumstances are studied comparatively, it is soon seen that these dilatations are far from following the same law. As to the quantities of heat acquired by the different bodies, when they are brought successively to different temperatures measured by the dilatations of any one of them, it is perceived that they are variable and unequally variable in each, while no one has yet succeeded in determining the relations which exist between these variations of capacity and these variations of volume.

It is evident therefore how difficult it would be to construct a perfect thermometer, that is to say a thermometer whose indications would always be proportional to the quantities of heat it absorbed, or in which the additions of equal quantities of heat would always produce equal dilatations. However, physicists imagined that they had found this normal thermometer in the gas-thermometer. This opinion was founded on the following considerations, namely, that if thermometers are made with air and several other solid or liquid substances, and graduated between 0° and 100°, with the supposition that the dilatation is uniform, when this graduation is prolonged beyond 100°, the air-thermometer is that which, for temperatures above 100°, will constantly indicate the lowest temperature; and while the others will indicate all the temperatures different from and higher than that which the gas-thermometer denotes, the latter will denote the same, whatever be the nature of the gas, whether, for example, it be air or hydrogen. This result of observation seemed moreover to be confirmed, by the opinion that was entertained that gases are subject to simple and general laws, and by the notions that were deduced from it as to their physical constitution, in which no part was attributed to the proper nature of the atoms, and which was made to depend solely upon heat, an agent common to all.

But the researches of M. Regnault, by demonstrating that the laws which regulate the constitution of gases are far from being so simple and general as had been believed, have rendered it inadmissible to regard the gas-thermometer as a normal thermometer. The indications of the gas-thermometers, like those of other thermometers, can therefore only be considered as more or less complicated functions of the quantities of heat.

In default of giving an exact measurement of the quantity of heat, the thermometer must at least always remain accurately comparable with itself, that is, must always furnish the same indication in identical conditions: and, further, it is necessary to be able to reproduce at will and always obtain instruments rigorously comparable. Now the mercurial thermometer which fulfills the first condition well, only fulfills the second very imperfectly. Thus two mercurial thermometers, regulated for the same fixed points of melting ice and of the boiling of water under the pressure of 0·760 millim., may present very considerable differences in the course beyond these fixed points, if

the envelopes of these thermometers are not formed of glass of the same nature, or the molecular condition of which is not the same. In fact, in any thermometer formed by a liquid or gaseous substance, the indications of the instrument depend upon the dilatation of that substance, and of that of the envelope. The dilatation of mercury being only seven times greater than that of the glass which encloses it, the variations presented by the different dilatations of different kinds of glass form very sensible fractions of the apparent dilatations of the mercury, and consequently influence notably the indications of the instrument. In the gas-thermometer, on the contrary, the dilatation of the gas being a hundred and sixty times greater than that of glass, the variations in the dilatations of the different kinds of glass do not sensibly influence the indications of the apparatus, and do not prevent the comparison of the instruments. At the same time, it is important to determine the conditions in which this instrument remains comparable.

Thus in the question of the measurement of temperatures, it is no longer sought to obtain an instrument which measures the quantities of heat, or the indications of which will be proportional to the temperatures. The pretension of the physicist is more modest; he asks only the possibility of constructing instruments which, in the same conditions of temperature, shall always give exactly the same indication, that is, which shall be comparable with each other and with themselves. The study of this desideratum is the object of that one of M. Regnault's memoirs which treats of the measurement of temperatures. He first discusses the gas thermometer, then the mercurial. Finally, he devotes an important part of his memoir to the measurement of temperatures by means of thermo-electric currents. We shall give in a future number, almost word for word, this last portion, which is very little known, and in which M. Regnault treats several interesting questions in thermo-electricity, with that same rigorous precision which he brings into all his researches. At present we shall confine ourselves to a rapid survey of the first two portions of the memoir, devoted to the gas and the mercurial thermometers, which form a whole, the more complete, that in regard to the measurement of temperatures, M. Regnaul thas been led to reject the employment of thermo-electric currents entirely, and that, consequently, the interest of that portion of his researches lies wholly in the details, and beside the principal subject.

Gas Thermometers.

There are two ways of employing gas as a thermometric substance. It may be placed in such conditions that the pressure to which it is subject remains constant, its increase of volume being observed, or the gas may be compelled to remain in the same volume while we observe the increase of its elastic force.

The first method requires the employment of a capillary tube, uniting a calibrated tube to the reservoir filled with air, which is exposed to the temperature to be measured. This arrangement allows of the calibrated tube being removed to a distance from the

enclosure of which we desire to know the temperature, this being indispensable; but it presents a great inconvenience when the apparatus is intended to measure elevated temperatures, for then the greater portion of the air is contained in the calibrated tube, and only a very small portion remains in the reservoir properly so called. The result of this is that the portion which will pass out by a new increment of temperature will be very small, and will be difficult to measure with sufficient precision in the calibrated tube. The apparatus thus becomes but very slightly sensible at elevated temperatures; M. Regnault has therefore rejected this arrangement of the air-thermometer.

In the second method the gas is maintained constantly in the same volume, and the elastic forces which it presents under different circumstances are the points determined. In this way, by knowing the variations produced in the elastic forces, we may calculate, by Mariotte's law, the dilatations which the gas would have experienced if the pressure had been kept constant. The apparatus founded on this method, besides being more easily manageable and of greater precision, has the advantage of presenting as much sensibility at high temperatures as at low. But in the employment of this method two important questions present themselves for solution. The first is, to know *if air-thermometers, filled with air at very different densities, are comparable with each other.* The second, *if gas-thermometers, filled with gases of different nature, proceed in agreement with each other when they have been regulated for the fixed points of* 0° *and* 100°.

The apparatus which M. Regnault has made use of to solve these two questions consists of two gas-thermometers, each composed of a glass balloon of from 700 to 800 cubic centimetres capacity, terminating in a curved capillary tube, which abuts, end to end, against another capillary tube communicating with the manometric apparatus. The union of the capillary tubes is effected by a brass tubulure bearing a rectangular appendix, into which is cemented a capillary tube, serving to put the apparatus in communication with an air-pump, by means of which the interior of the apparatus may be dried, and the different gases to be operated on be introduced. The balloons themselves are plunged into a heating apparatus filled with oil, which is constantly agitated in order to maintain an equal temperature throughout the bath. We pass over the details of the experiments, and the enumeration of the precautions taken to dry the interior of the apparatus properly, to maintain the temperature as nearly stationary as possible, and to determine satisfactorily the levels of the mercury in the manometers. The experiments were conducted in the same manner, whether it was wished to compare the course of an air-thermometer with that of a thermometer filled with some other gas, or to compare the course of an air-thermometer charged with air having an initial elastic force of about 760 millims. at 0°, with that of a similar thermometer filled with air of a less or greater density.

The tables contain the results of the experiments made on the

comparison of air-thermometers charged with air under an initial pressure of 762, 553, and 438 millims. In a subsequent series of experiments, the initial elastic force of the air was carried as far as 1486 millims. The experiments were made from 0° up to 325° of the scale made with the graduation of the instruments between 0° and 100°. All these thermometers proceeded in sensible agreement, even when the air which each contained was under very different pressure, so that it may be admitted with all certainty, *that the air-thermometer is a perfectly comparable instrument, even when charged with air of different densities.*

Two thermometers charged, one with air, the other with hydrogen, with an initial elastic force of 754 millims. at 0°, proceeded with perfect agreement from 0° to 325°. The same was the case with two thermometers of carbonic acid gas, which, in two series of experiments, proceeded in equally perfect agreement with the air-thermometer. The last was in both cases charged with air of an initial elastic force of 742 millims., while in the carbonic acid gas thermometers the initial elastic force of the gas was, in one 741 millims., and in the other only 464 millims. It must be observed that the temperatures were calculated, in the various experiments, by taking for the coefficient of dilatation of air 0·003665, for that of hydrogen 0·003652, and for that of carbonic acid 0·003695 when it was under the pressure of 741 millims., and 0·03682 when it was under the pressure of 464 millims.

Two series of experiments made on the comparison of a normal air-thermometer with a thermometer of sulphurous acid gas, under the initial pressure of 762·17 millims. for the air, and successively of 751·47 millims. and of 588·70 millims. for the sulphurous acid, showed a very notable difference of course between the two instruments. In the first series 0·003825 was taken for the coefficient of dilatation of the sulphurous acid gas, and in the second 0·003794. The sulphurous acid thermometer fell behind the air-thermometer from the point of 100°, and the differences increased regularly with the temperature. Thus the mean coefficient of dilatation of sulphurous acid gas diminishes in a very marked manner with the temperature measured in the air-thermometer; in fact, the value of this mean coefficient for each degree Centigrade is found to be

from $\overset{\circ}{0}$	to $\overset{\circ}{98}$·12	0·0038251
...	to 257·17	0·0037923
...	to 310·31	0·0037893

The Mercurial Thermometer.

The air-thermometer is the only instrument, especially for elevated temperatures, which can be used in exact experiments, but the use of it is difficult; there are even circumstances in which it is impossible to employ it. Then it becomes necessary to use a mercurial thermometer; and then a direct comparison must be made of this instrument with the air-thermometer, in order that its indications may be transformed into those of the normal thermometer.

Dulong and Petit had already made this comparison, and calculated a table which permitted the necessary transformation; but this table is inexact, even for the particular mercurial thermometer which they employed, because their experiments were calculated with much too high a coefficient of dilatation—the coefficient 0·375 of Gay-Lussac.

M. Regnault, after he had made a great number of experiments on this subject, perceived that the different mercurial thermometers were not comparable, either because they had not been constructed of the same kind of glass, or because they had been blown in a different manner. At the same time, he was desirous of ascertaining if mercurial thermometers, constructed of the same kind of glass, would, although blown in a different manner, proceed sufficiently in agreement to allow of their being regarded as comparable. If this circumstance were realized, it would suffice to make, once for all, the comparison of one of these thermometers with the air-thermometer, and to admit the same table of correction for all similar instruments. To settle this question, M. Regnault executed a long series of experiments with the aim of comparing with the air-thermometer, not only mercurial thermometers formed of an identical quality of glass worked in a different way, but also those made of the different kinds of glass which are met with commercially in France, and which are employed in physical apparatus. The mercurial thermometers employed in these experiments were *overflow*-thermometers (*thermomètres à déversement*); they are more easily constructed than the ordinary thermometers with graduated stem, and present the great advantage, that it is always easy to keep the whole of the column of mercury in the bath. In order to construct overflow-thermometers, the mercury must be boiled in the instruments several successive times and with great care, after which they are left to cool with the recurved point of the capillary tubes kept in a bath of mercury previously heated. Then the reservoirs and capillary tubes are enveloped in melting ice, the open point remaining plunged in the basin of mercury, and it is easily seen that the thermometer has acquired the temperature of 0° when the column of mercury remains stationary at the extremity of the capillary tube. The mercury which escapes in consequence of the elevation of temperature is collected in a little empty capsule, and weighed with great care. To obtain the weight of the mercury which filled the thermometer at 0°, the weight of the mercury which has escaped through the elevation of the temperature above 0° is added to that of the weight of the thermometer itself, subtracting from the whole the weight of the empty apparatus. Then, knowing the weight of the mercury at 0°, and weighing with care the quantity which escapes in proportion as the temperature is elevated, it is easy to conclude from it the temperature itself, and to compare it to that which would be given by a thermometer constructed of the same glass but with a graduated stem. M. Regnault successively analysed with care the indications of the thermometer with graduated stem and the overflow-thermometer; he shows that by taking into ac-

count all the circumstances, the dilatation of the bulb, that of the tube, &c., the indication which the first of these thermometers would give is identical with that which the second would give in the same circumstances, provided that their reservoirs were of the same capacity.

Convinced by this analysis that he might employ indifferently either form of the thermometer, M. Regnault, from the motives we have already indicated, preferred the method of overflow, as susceptible of much greater exactitude; the greatest losses which he could perceive in his weighings never exceeded 3 or 4 milligrammes, which is exceedingly inconsiderable for the temperatures to which his experiments were carried.

He tried in succession thermometers made with the flint-glass of Choisy-le-Roi, with common glass, with green glass, and with Swedish glass. He took care to have each kind of glass analysed by a skilful chemist, and the analyses were made of the reservoirs themselves of the thermometers.

The flint-glass of Choisy-le-Roi always presenting exactly the same composition, on account of the very particular care taken in the manufacture, is uncommonly well-fitted for comparative experiments. Three thermometers were prepared with this kind of glass; one had the reservoir made of a flint-glass tube about 14 millims. in internal diameter, and this was soldered to a capillary tube of the same glass; the second was obtained by blowing a spherical reservoir on a capillary tube of flint-glass; and the third was formed of the same capillary tube worked in the lamp so as to produce a cylindrical reservoir. This last exhibited a rather larger proportion of silica in the analysis, which was probably occasioned by the long working in the lamp having driven off some of the other ingredients of the glass by volatilization.

Observed successively in their course, compared with air-thermometers, the three mercurial thermometers, made of Choisy-le-Roi flint-glass, proceeded in sensible agreement from 0° up to 325°, and experiment showed that the same corrections may be applied to them to bring their indications to those of the air-thermometer. But though the law of dilatation did not differ in the glass of the different thermometers, this was not the case with the absolute dilatation, which was very different for each of the reservoirs, the first of which exhibited a sensibly smaller coefficient of dilatation than the other two.

Several thermometers constructed of common glass, but some with cylindrical, others with spherical reservoirs, and some formed of tubes or little globes soldered to capillary tubes, were in like manner compared with the air-thermometers. It was found that they differed considerably in their course from the thermometers with flint-glass envelopes, and that these two kinds of thermometers could not be regarded as comparable. The dilatations of common glass between 0° and 100° varied in a very marked manner with the difference of composition, and above all according to the manner in which the glass had been worked. Now there are great differences in the composition of common glass, which is not manufactured with

the same care as the Choisy-le-Roi flint-glass. At the same time, on comparing together the results obtained on thermometers of common glass, the same conclusion is arrived at as for the thermometers made of the flint-glass of Choisy-le-Roi, namely, *mercurial thermometers constructed of the different varieties of common glass, which are at present used in the manufacture of chemical instruments, do not proceed in strict accordance beyond the fixed points which have been used to regulate their scales; but the differences are so small that they may be neglected in most experiments, especially if care be taken to reject glasses which contain a sensible quantity of lead, which may readily be detected when they are worked in the lamp.*

The thermometers made of green glass, similar to that which is used in Paris for organic analyses, and of a Swedish glass, remarkable for its infusibility, gave results sufficiently analogous to those which had been obtained from the more numerous observations made with the other thermometers. Only, the coefficient of dilatation of these two kinds of glass, which, besides, were of a different chemical composition, was not the same as that of the other glasses.

After having demonstrated that thermo-electrical currents cannot be used for the measurement of temperatures, in a portion of his work of which we shall not speak at present, since we shall copy it verbatim, M. Regnault terminates his memoir with some general conclusions, of which the following is a summary.

The air-thermometer is the only instrument of measurement which can be applied with confidence to the determination of elevated temperatures; this is the only one M. Regnault will employ when the temperatures exceed 100°.

The air-thermometer should be founded on the measurement of the changes of elastic force which a given volume of air undergoes when it is brought to different temperatures. It is requisite, as far as possible, to arrange the air-thermometer so as to determine directly by experiment the elastic forces between 0° and 100°, the reservoir being plunged into melting ice or kept in the vapour of boiling water. But if, from the arrangement of the apparatus, the direct determination of the two fixed points of the thermometric scale is impossible, as sometimes happens, it becomes necessary to take the point of departure of the air-thermometer at the temperature of the surrounding medium, ascertained by a mercurial thermometer, and then to deduce, by calculation, the elements proper to the apparatus for the temperature of melting ice.

When the thermometer contains air having an elastic force of 760 millims. at 0°, and if it does not exceed the temperature of 350°, the elastic force of the air within will not become greater than 1720 millims.; there will be no fear, therefore, of a permanent deformity of the envelope being produced. But at higher temperatures there may be a fear of this alteration of form;—1, because the internal pressure becomes considerable; 2, because the glass may undergo a sensible softening. It is consequently advisable to introduce into the thermometer air having a weaker elastic force when

the instrument is intended for the measurement of very high temperatures. If, for example, the air at 0° has an elastic force of 300 millims., it will acquire at 500° an elastic force of 850 millims., which only exceeds the external pressure by about 90 millims.

When operating at high temperatures, by a peculiar arrangement of the apparatus it is possible to avoid the danger of an alteration of the form of the reservoir when there is only one temperature to determine. But there is always a cause of uncertainty arising from ignorance of the law of dilatation of the envelope, which may be of platinum instead of glass, in particular in the case of an air-pyrometer; but this cause never leads to very considerable errors, as may be seen by experiments made up to 350° on air-thermometers with a glass envelope. The errors may become more considerable, and even rise to several degrees when the temperature exceeds 300°, if, the dilatation of the envelope between 0° and 100° being unknown, *the apparent coefficient of dilatation* be deduced from the elastic forces which the gas presents from 0° to 100°.

M. Regnault indicates, in concluding, the possibility of employing with advantage a thermometer of mercurial vapour, in a great many cases in which the experiments do not require very minute precision; that is to say, this instrument would be a pyrometer intended to measure temperatures above that of the boiling-point of mercury. The mercury ariving at ebullition drives the air completely out of the apparatus, and the vapour of mercury, behaving now as a permanent gas, dilates so as to remain in equilibrium with the external pressure. By means of a peculiar arrangement, it is possible to extract the mercury which has been condensed on the sides and determine its weight, after the apparatus has cooled down to the temperature of the surrounding medium. By means of a very simple formula, and by admitting for mercurial vapour the same coefficient of dilatation as that of air, the temperature to which the apparatus has been exposed may be determined. It is requisite only, in order to avoid the oxidation of the mercury at the beginning of the experiment, when the mercurial vapour has not yet driven the air out of the apparatus, to place in the vessel a little oil of naphtha, which first drives out the air and then is itself expelled by the mercurial vapour.

M. Regnault announces that he intends to execute some experiments, by this method, at the porcelain manufactory of Sèvres; we do not know whether he has realized his project; the results, if he has obtained any, are still unknown to us.

VIII. *Intelligence and Miscellaneous Articles.*

NEW PROCESS FOR EXTRACTING SUGAR FROM THE SUGAR-CANE. BY M. MELSENS.

THE following account of the new and important method of extracting sugar from the sugar-cane, is abridged from the first of two long articles recently published in the *Courier de l'Europe*.

The great difficulty which has been experienced up to the present time in the preparation of sugar, has been owing to the rapidity with which it, when dissolved in water, alters by exposure to the air in hot climates. It must, however, be clear, since the cells of the sugar-cane are themselves full of sugar dissolved in water, and this solution can be kept for a long time in them, without undergoing any alteration at all, that if the same conditions which exist in nature could only be obtained in practice, there is no reason why an artificial solution of sugar may not be kept unaltered for a considerable space of time; or in other words, why water should not be used for the purpose of dissolving the sugar out of the crude juice expressed from the cane.

The difficulties, indeed, are not owing to the sugar or to the water, but to the air, and the ferments produced by its action on the crude sap of the sugar-cane. The object of M. Melsens was, then, to exclude the air from the sap when extracted from the cane, and to prevent the formation of any ferments which might change the character of the saccharine matter. This he has succeeded in doing by availing himself of the well-known affinity of sulphurous acid for oxygen gas. Sulphurous acid, however, alone was found not to answer the purpose; the sulphuric acid, produced by the absorption of oxygen by sulphurous acid, acting on the sugar, converts it into grape-sugar. This difficulty has been overcome by using sulphurous acid combined with a powerful base, which, as the sulphurous acid is converted into sulphuric acid, combines with the latter and forms an insoluble salt.

The acid sulphites, and more especially the bisulphite of lime, were employed by M. Melsens for the double purpose of preventing fermentation by the action of the sulphurous acid, and of neutralising the sulphuric acid as fast as it formed by means of the lime.

Sugar-candy dissolved in cold water containing bisulphite of lime, even in excess, crystallized entirely, and without undergoing any change, by spontaneous evaporation, at a low temperature. Several other experiments of the same nature, but differing in their details, always gave the same result; in each the sugar crystallized out by spontaneous evaporation, without any loss either in quantity or in quality, and without any appearance of molasses. In these experiments, the sugar dissolved in water, containing bisulphite of lime in excess, was boiled, and then left to evaporate, sometimes after being filtered, sometimes without any filtration at all.

From the experiments which M. Melsens has made with bisulphite of lime, it is probable that if a cold solution of this salt were to be poured on the sugar-cane grinder, so as to mix with the juice the moment it is expressed from the cane, the sugar might be kept for some time, and might be exposed to the heat necessary for its clarification without any sensible loss or deterioration.

But this same salt also possesses the property of coagulating, at a temperature of 212°, milk, white of egg, blood, and yolk of egg mixed with water. At a temperature of 212°, bisulphite of lime acts as a clarifier. It separates the albumen, caseum, and other similar

azotized matters which are found in the sugar-cane. This separation is effected without appreciable loss in the quantity, or deterioration in the quality, of the sugar.

Bisulphite of lime, moreover, rapidly and tolerably effectually bleaches the coloured substances found in the sugar-cane; it prevents the formation of other coloured matters produced by the action of air on the pulp of the cane; it also stops the production of those which are formed during evaporation, and above all of those which require for their development the joint action of air and a free alkali.

It seems that coloured substances which, under ordinary circumstances, are formed spontaneously by the exposure of the pulp of the sugar-cane to the air, never make their appearance when bisulphite of lime is employed. By evaporating, at a low temperature, bisulphite of lime mixed with—1, a common solution of sugar; 2, the crude sap of the sugar-cane; 3, the juice of beet-root; no coloration was produced. By an evaporation of the same substances at a high temperature, the coloration was scarcely visible; indeed, with red beet-root the colour was completely destroyed, and the sugar obtained was perfectly white.

It seems, then, that bisulphite of lime can be employed in the extraction of sugar:—1st, as an antiseptic, preventing the production and action of any ferment; 2nd, as a substance greedy of oxygen, opposing any alteration that might be caused by its action on the juice; 3rd, as a clarifier, coagulating at a temperature of 212° all albuminous and other coagulable matters; 4th, as a body bleaching all pre-existing coloured products; 5th, as a body opposing itself in a very high degree to the formation of coloured substances; 6th, as a base capable of neutralising any hurtful acids which might exist or be formed in the juice, and substituting in their place a weak inactive acid, namely, sulphurous acid.

M. Melsens is of opinion that sugar can be obtained from the sugar-cane with no other source of heat than a tropical sun, excepting only for the purpose of clarification; indeed, the bisulphite of lime prevents the crude juice of the cane, or the syrup obtained therefrom, from undergoing any changes; great rapidity in the process of crystallization, indispensable at present, becomes by using this salt unnecessary; and more than this, the quantity of sugar which is now lost in the bagasse, in consequence of the impossibility of washing it out unchanged, can be all collected by being dissolved in water charged with bisulphite of lime.

The only objection that can be made to the above process is, that the sugar obtained by means of bisulphite of lime has a sulphurous taste; this is true, but the taste is completely lost—1st, by crushing the sugar and exposing it to the air, whereby the little sulphite of lime which there may be is converted into a tasteless sulphate; 2nd, by exposing the sugar to an atmosphere containing ammonia; if this is done the sugar acquires a very agreeable flavour of vanilla, but is apt to become a little discoloured; 3rd, by clarifying it until it loses 10 per cent. of its weight; by this process a pure white sugar can be obtained, which will bear comparison with any sample produced at

present. The last is the process recommended to be used on a large scale. The quantity of sugar fit for the market which can be obtained from the sugar-cane by adopting bisulphite of lime, as above recommended, is at least double that obtained by the usual processes.

In consequence of M. Melsens having made all his experiments on the sugar-cane at Paris, and therefore on a small scale, he is not able to state how bisulphite of lime can best be used in the large colonial sugar manufactories, but is compelled to leave the application of the principles on which his method depends to the intelligence of the manufacturers themselves.

In the preparation of beet-root sugar bisulphite of lime is quite as useful as in the extraction of cane-sugar; the way in which it is to be employed in the former is fully explained in the second article published in the 507th number of the *Courier de l'Europe,* to which we must refer those among our readers who desire any further information on the subject.—*Gard. Chron., Dec.* 15, 1849.

MR. S. M. DRACH'S THERMOMETRIC SCALE.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

Having lately had occasion to advert to the superior practical utility of Fahrenheit's thermometric scale, as not ordinarily requiring decimal divisions and negative degrees like its congeners, it occurred to me that a superior scale was feasible by dividing the distance from $-40°$ Fahr. $=-40°$ Cent. $=-32°$ Reaum. unto the boiling-point $+212°$ Fahr. $=+100°$ Cent. $=+80°$ Reaum. into one thousand degrees, placing zero at the former temperature. Thus one of my degrees $=0°\cdot252$ Fahr. $=0°\cdot140$ Cent. $=0°\cdot112$ Reaum.; and except for some chemical purposes and extraordinary arctic temperatures *no negative degrees would be necessary.*

If the zero were placed at

$$-38° \text{ Fahr.} = -38°\cdot8 \text{ Cent.} = -31°\cdot1 \text{ Reaum.,}$$

$$\text{my degree} = \frac{1}{4} \text{ Fahr.} = \frac{10}{72} \text{ Cent.} = \frac{1}{9} \text{ Reaum.}$$

easier for marking off the point of melting ice in whole degrees. Some of the prominent points in these two scales are as follows:—

Mercury freezes	− 39° F.	= +	4°·0 (1)	or −	4° (2)
Fahrenheit's zero........	+ 0	=	158·7	= +	152
Ice melts	+ 32	=	285·7	=	280
Water's max. density....	+ 39	=	313·5	=	308
Medium temp. 10° C.....	+ 50	=	357·1	=	352
Equatorial temp.........	+ 84	=	492·1	=	488
Blood heat	+ 98	=	547·6	=	544
Greatest heat felt, say....	+150	=	754·0	=	752
Alcohol boils	+174	=	849·2	=	848
Water boils	+212	=	1000·0	=	1000
Mercury boils	+655	=	2758·0	=	2772
	+672	=	2825·4	=	2840

In a small tract printed ten years ago, I showed that the idea of an absolute zero of cold expressible by a thermometer (Library of Useful Knowledge, *Heat*, ch. viii.) involved some fallacy. Fill a cylindrical tube with a heat-dilatable fluid, *e. g.* air, and continuing the divisions to each end of the tube, we should find that as air expands $\frac{1}{480}$th in volume per degree from 32° Fahr. to 212° Fahr., the bottom of the tube would have $-480+32=-448°$ Fahr. marked on it. Fill such a tube with mercury, which expands $\frac{1}{9990}$th per degree betwixt the same extremes, and the bottom of the tube will indicate $-9958°$ Fahr. Now could the substance ever contract to the bottom of the tube, its density must become infinitely great, as it then forms an infinitesimally thin stratum. May it therefore not be concluded that every substance has its own minimum temperature for maximum density, above or below which it expands just as water on each side of 39° Fahr.; the latter fluid having the lion's share of this peculiarity, just as magnetism is ordinarily visible in iron above all other metals, resembling in some sort the elective affinity so common in chemical combinations?

London, Dec. 6, 1849. S. M. Drach.

ACTION OF HYDROCHLORIC ACID ON THE HYDRATES OF OIL OF TURPENTINE. BY M. H. DEVILLE.

If one of the hydrates of oil of turpentine be treated with hydrochloric acid, water is separated and camphor formed, that is to say a hydrochlorate of the oil combined with water. The product is not similar to the solid camphor of turpentine, either in composition or chemical properties, but it appears to be identical with the camphor of oil of lemons. Its composition is as follows:—

	Experiment.			Calculation.	
	I.	II.	III.		
C	57·2	57·7	..	C^{10}	57·2
H	8·7	8·8	..	H^{9}	8·6
Ch.	34·1	33·7	34·4	Ch.	34·2
	100·0	100·2			100·0

Its melting-point is the same as that of the camphor of oil of lemons, that is to say about 113° F.; when this hydrochlorate is heated, it is decomposed, and loses hydrochloric acid. Treated with potassium, it gives rise to a fluid colourless oil, the odour of which may be mistaken for that of the oil of lemons. When the decomposition is effected at the lowest possible temperature, the product obtained has the sweet odour of lemon-peel; when, on the other hand, the camphor is made to boil, so as to deprive it of acid as much as possible, so that the decomposition by the potassium takes place at a high temperature, the odour of the product resembles that of citren, or the substance resulting from the action of lime on the oil of lemons.

Analysis gave the following results:—

	Experiment.	Calculation ($C^{10} H^8$)
Carbon	88·04	88·24
Hydrogen	11·82	11·76
Loss	0·14	..
	100·0	100·0

which is the same composition as oil of turpentine and oil of lemons.
—*An. de Ch. et de Phys.*, Septembre 1849.

ON THE DRY DISTILLATION OF CAMPHORATE OF LIME.

MM. Gerhardt and Lies-Bodart have found that the dry distillation of the salts of lime and organic acids produces interesting results, which serve as a transition between different series: with the exception of suberic acid, monobasic acids only have been examined in this respect: the authors have made experiments with camphoric acid, the characters of which as a bibasic acid are very distinct. They have ascertained that camphorate of lime yields by heat an essential oil, possessing the odour of oil of peppermint, and that its composition is similar to that of the acetonides of the monobasic acids. This oil is represented by $C^9 H^{14} O = 2$ volumes of vapour. It is remarkable on account of the metamorphoses which it undergoes by anhydrous phosphoric acid, which converts it into cumen, $C^9 H^{12} = 2$ volumes of vapour; its identity with which the authors have ascertained by the easy formation and analysis of sulphocumenate of barytes, $C^9 H^{14} BaSO^2$. This reaction associates the camphoric to the cuminic, and consequently to the benzoic series. As camphoric acid, though bibasic, offers no exception to the general rule, it is probable that the anomaly which is presented by suberic acid may be got rid of by more complete researches.—*L'Institut*, Novembre 7, 1849.

CYANIDE AND NITRURET OF TITANIUM.

W. Wœhler has examined the cubic crystals obtained from the iron blast furnaces and hitherto supposed to be metallic titanium; he has found that they are formed of cyanide and nitruret of titanium; they contain 18 per cent. of nitrogen and 4 per cent. of carbon, their formula being $TiC^2 N + 3TiN$ (or $TCy + 3Ti3N$).

M. Wœhler also states that the titanium obtained by M. H. Rose is a nitruret of titanium containing 28 per cent. of nitrogen, its formula is $Ti^3 N^2$.

The cubic crystals fused with hydrate of potash yield ammoniacal gas; these crystals, heated in a current of chlorine, produce a liquid chloride of titanium, and a very volatile crystallized body, which is a compound of cyanide and chloride of titanium; the latter substance may be obtained by exposing chloride of titanium to gaseous chloride of cyanogen. By heating the cubes to redness in a current of aqueous vapour the latter is decomposed, hydrogen gas is obtained, as already observed by M. Regnault; but there are also produced

ammonia and hydrocyanic acid. The titanic acid which is left possesses the same octohedral form as anatase; it is *artificial anatase*. M. Wœhler succeeded in forming cubic crystals by heating a mixture of titanic acid and ferrocyanide of potash in a forge. As to the simple nitruret, it is very easily obtained by heating titanic acid to redness in a current of ammoniacal gas, or of cyanogen or hydrocyanic acid gas. This substance always possesses a remarkable metallic lustre.

By the same process M. Wœhler has obtained the nitrurets of several other metals, with which he is at present occupied.—*L'Institut*, Novembre 7, 1849.

ANALYSIS OF CAST IRON. BY F. C. WRIGHTSON, ESQ.

The effect of phosphorus in producing what is termed "cold short iron" has long been admitted. That the use of the hot blast occasioned an *increase* in the phosphorus of the iron has, however, so far as I am aware, never been suspected; at all events, the fact has never been announced. It was with the view of elucidating this point, and also of furnishing more complete analyses of cast iron than had yet been done, that I undertook, in the autumn of last year, a series of analyses, the results of which I communicated to the Birmingham Philosophical Society; and they were afterwards published in No. 3 of the 'Quarterly Journal of the Chemical Society.' It is necessary to mention this for reference, the results as to phosphorus being partly deduced therefrom. I have endeavoured in the present analyses more completely to establish the fact, that the hot blast increases the "cold shortness" of iron, by occasioning the reduction of a larger amount of phosphoric acid. I have also paid some attention to the different states in which carbon appears to be combined, which I shall point out; a fact previously noticed by Bromeis and Karsten.

Karsten states, that if the ore contains phosphoric acid, he has invariably found it as phosphorus in the slag. I have appended a number of analyses of the ores from which the cast irons were made. Phosphoric acid exists in most of them to a considerable extent. They are never smelted separately, but indiscriminately, two, three or four kinds at once.

Karsten states, that the artificial graphite, obtained by dissolving gray iron in an acid, must be considered as a compound of carbon and iron. He does not, however, give any analyses of this substance. I have examined some specimens obtained by treating the irons C. I., C. III., H. VII. and H. VIII., and subjoin the analyses of three; (see p. 74.) I had not sufficient material to complete that of No. VII. Those of the higher numbers contain carbon and iron (besides small quantities of silica, &c.) in nearly their equivalent ratios; whilst the lower numbers appear to be mixtures of silicates of oxide of iron, &c., with varying proportions of carbon. It would seem, in fact, as though the carbon of the latter had separated from the molten iron in an uncombined state, and that of the former as a carburet. I say, "separated from the *iron*," to distinguish it from that which evi-

dently remains *combined* in the iron, with it or other elements, or it could not be eliminated as a hydrocarbon when the iron is treated with acids; but the nature of these combinations of carbon would be an ample subject for distinct investigation. In distinguishing between the carbons existing in the specimens, I would only observe that the *b*-carbon is separated by acids, and may be collected on a filter, whilst the *a*-carbon cannot be so collected; it escapes as a hydrocarbon, having a powerful alliaceous odour, and may be partially collected as a liquid. I have only further to notice the invariable presence of sodium or potassium, or both, in these irons. Karsten states that they have not been found as constituents of iron hitherto. As they are present in only a few of the ores, they are probably, in part at least, derived from the smelting materials.

Method of Analysis for the Irons.—The specimens were easily broken into small pieces in a steel mortar. In one portion of from 20 to 30 grs. the sulphur and phosphorus were determined. In a second quantity, all the other constituents were determined except the carbon. On being treated with HCl and warmed, the iron is quickly acted on, and in a few hours dissolved, leaving black flakes and particles floating in the liquid. These were collected on a filter previously dried at 212° and weighed. After well washing, until no trace of HCl remained, the filter was again dried and weighed. The increase was carbon principally, with small quantities of silicates of oxides of iron, lime, &c., and in the numbers VII. and VIII., iron in an *equivalent* proportion to the carbon. In these latter, as also in the numbers I. and III., the silica, iron, &c. of the substance separated by the filter were determined by fusing it with nitrate of potash mixed with twice its weight of carbonate of soda; the iron, &c., separated in the usual manner, gave the carbon by loss. This was afterwards verified in two instances by a direct determination of the carbon in a combustion-tube. The iron, &c., separated in the above manner, was added to that obtained from the *solution*. The carbon was designated *b*, and being deducted from the *entire* quantity found in the iron by the method to be detailed, gave the quantity of carbon designated *a* for the reason before named. The filtered liquid and washings, evaporated to dryness and again treated with acid and water, usually left a minute portion of silica, which was separated, weighed, and added to the former quantity. A current of sulphuretted hydrogen being passed through a small quantity of the solution, it in no case gave any other than a milk-white precipitate of sulphur. After being carefully freed from this and from SH, it was returned to the main solution, NO^5 added, and boiled until all the iron was peroxidized, and ammonia added gradually until the solution only faintly reddened litmus, and nearly all the iron was precipitated. A little neutral benzoate of ammonia separated the last portions of the peroxide of iron. The precipitate, after well washing, was dried, ignited, weighed and examined for manganese by fusion with nitrate and carbonate of potash; for chrome and alumina by dissolving in ClH, and precipitating with caustic

potash in excess; only minute traces of alumina were occasionally found. But the peroxide had in one or two instances to be redissolved and the manganese separated afresh. This occurred when ammonia had been added to the solution in excess, and a few drops of HCl again added, which were not sufficient to neutralize or acidify the whole of the solution, which from being very bulky required care in neutralizing. When the entire solution, after attaining the requisite degree of acidity, was transferred from one vessel to another so as to obtain a uniform mixture, then no trace of manganese was found with the iron. From the amount of peroxide the per-centage of iron was calculated. Before proceeding to separate the manganese, the solution and washings were evaporated to dryness, and the salts of ammonia driven off by ignition to redness. This, I had found from repeated trials, was absolutely necessary, in order to separate the whole of the manganese by hydrosulphate of ammonia. After ignition, the residue was always of a brown colour from the peroxide of manganese; a drop or two of HCl dissolved this. Ammonia and hydrosulphate of ammonia were then added, and the solution allowed to stand for several hours, and gently warmed. The sulphuret of manganese thus separated was converted into sulphate of manganese, from which the percentage of manganese was calculated. In one or two cases, where small quantities of nickel and cobalt were present, these were left as sulphurets on the filter, when the sulphuret of manganese was dissolved by dilute sulphuric acid. The solution, after getting rid of the excess of hydrosulphate of ammonia, was neutralized, and oxalate of ammonia added. The lime thus separated was converted into carbonate, and from it the proportion of calcium deduced. A drop or two of the solution with phosphate of soda occasionally indicated *very minute* traces of magnesia; these were overlooked. After separating the lime, the solution was evaporated to dryness, ignited in a platinum capsule, and the residue, consisting of the alkaline chlorides, weighed; a few drops of solution of bichloride of platinum being added to the moistened salts, the potash was separated (when sufficient in quantity to weigh) in the usual manner; the weight of the chloride of potassium calculated, and deducted from the weight of the mixed chlorides; the loss gave the weight of the chloride of sodium.

Determination of the Sulphur and Phosphorus.—The iron, reduced to fragments, was treated with fuming nitric acid and gently warmed; the reaction was violent. The nitrous fumes given off contained no trace of SH. The solution was evaporated to dryness, and the dried mass treated with HCl and water; to a little of the filtered solution a drop or two of BaCl were added; if, after standing several hours, any precipitate or cloudiness was occasioned, the whole of the solution was treated in like manner, and the sulphate of barytes allowed fully to subside, separated, detached from filter, &c., and the weight of sulphur calculated. The excess of baryta was separated from the solution by a few drops of dilute SO^3, and afterwards supertartrate of ammonia added in sufficient proportion to

prevent the precipitation of the iron by ammonia, which was then added in considerable excess, and a current of SH passed through the solution for several hours. The solution was then allowed to stand in a warm place until it had become of a clear light yellow colour, when it was quickly filtered and washed with water containing a little hydrosulphate of ammonia. The solution was then evaporated to dryness, the ammoniacal salts driven off by ignition, and the residue, consisting of phosphoric acid, with minute portions of lime, alumina and alkalies, fused with a little carbonate of potash and soda. [This fusion sometimes required repeating once or twice before the whole of the phosphoric acid was detached as tribasic alkaline phosphates.] The PO^5 was then determined in the usual manner as the ammonio-phosphate of magnesia.

Determination of Carbon.—The iron was reduced to a moderately fine powder by being turned in a lathe. A still finer powder was obtained from this by sifting through lawn. About 30 or 40 grs. of this were rubbed for a considerable time in an agate mortar along with about its own weight of hard white sand, which had been previously mixed with a little oxide of copper, and ignited to destroy any traces of organic matter. When an almost impalpable powder was thus obtained (care being taken to avoid any loss by rubbing over a sheet of glazed paper), it was mixed with 6 or 8 times its bulk of chromate of lead, and introduced into a combustion-tube, at the extreme point of which a few grains of chlorate of potash had been placed; the combustion was conducted with the usual precautions, and the carbon calculated from the carbonic acid formed, the latter of course being passed through a CaCl tube previous to absorption. Trials were made with the substances remaining in the combustion-tube, to ascertain if any trace of carbon remained therein; but no trace of carbonic acid was obtained by attempted recombustion of substances again powdered.

Nitrogen was sought for in the iron by mixing the powder with soda-lime, &c.; but none, or only such minute traces of ammonia were obtained as to render it questionable if they might not have been derived from the atmosphere of the laboratory. I append a comparison of the proportions of phosphorus in the hot and cold blast iron, taken from this and the series of analyses before alluded to. No. V. is generally considered the best in quality for forge iron, Nos. VII. and VIII. being too brittle, this in all probability arising from the different mode in which the carbon is combined. The difference in appearance between these two irons and the others is remarkable, being of a much whiter and finer fracture, with only a few grayish specks in the centre of the "pig." In No. V. these are much increased, and the colour is of a mottled gray; in Nos. IV. and III. the colour is still darker. These different appearances can be caused in the same iron simply by altering the time of cooling; if, for example, when the iron of the gray or mottled kind is running, a portion just set be thrown into cold water, it becomes precisely similar to No. VII. or VIII.

Analyses of Seven Specimens of South Staffordshire Iron Ores.

	I. Binds.	II. Blue flats.	III. Penny earth.	IV. Gubbin.	V. White iron stone.	VI. White Tree. (Also called grains.)	VII. Black Tree. (Also called grains.)
Water	4·41	2·63	0·50	1·02	0·58	0·54	1·42
Organic matter		1·67	1·81	1·05	0·41	2·19	
Silica	42·82	10·15	8·18	14·57	17·00	15·35	9·04
Alumina	13·72	trace	2·40	4·91	12·92	11·19	5·99
Carbonic acid	14·79	30·82	33·36	27·06	24·51	25·79	30·10
Protoxide of iron	19·30	49·41	50·72	44·50	39·14	41·18	50·71
Protoxide of manganese	0·25	..	0·80	0·51	2·66	2·83	1·22
Protosulphuret of iron	..	..	..	..	..	..	trace
Lime	trace	2·62	1·61	1·75	0·26	0·61	1·72
Magnesia	..	1·47	..	..	..	..	0·37
Potash	..	..	..	..	..	..	0·83
Soda	..	..	..	4·77	..	..	0·59
Chlorine	traces	traces	trace	trace	traces	trace	trace
Phosphoric acid			1·00	0·32	0·95	0·90	trace
Sulphuric acid			trace	trace	0·28	trace	..
	95·29	98·77	100·38	100·46	98·71	100·58	100·99

Analysis of iron ore from Port Madoc, South Wales.

Water	5·10
Organic matter	1·14
Silica	17·56
Alumina	15·06
Carbonic acid	12·73
Protoxide of iron	20·32
Peroxide of iron	23·40
Peroxide of manganese	4·27
Nickel and cobalt oxides	0·41
Lime	1·18
Potash, Soda	0·39
Phosphoric acid	trace
Sulphuric acid	trace
	101·56

Analyses of Ten Specimens of Cast Iron, made from South Staffordshire Iron Ore, chiefly West of Dudley.

Iron from Cold Blast.

	I.	III.		IV.		V.
Iron	94·10	96·57		94·53		94·42
Combined carbon (a)	1·87	0·95	C.		C.	
Uncombined carbon (b)	1·92	1·67	1·98	3·71*	2·73	4·05
Silica	1·30	0·51				
Manganese	1·12	1·16		0·33		0·94
Cobalt	trace	..		..		trace
Chromium	trace					
Calcium	0·05	trace		0·25		0·16
Sodium	0·16	trace		0·30		0·34
Potassium	trace	0·42				
Sulphur	trace	0·11		0·05		trace
Phosphorus	0·21	0·36		0·03		0·36
	100·73	101·75		99·20		100·27

Iron from Hot Blast.

		I.		III.		IV.		V.		VII.	VIII.
Iron		89·53		92·98		93·84		92·90		95·23	95·80
Carbon	C.		C.		C.		C.		C (a)	1·77	2·72
Carbon	3·27	7·93	3·11	6·51	2·98	5·54	0·87	6·83	C (b)	0·49	0·26
Silica, &c.										0·31	0·11
Manganese		1·71		1·30		0·72		0·62		0·34	0·54
Calcium		0·11		trace		0·34		0·06		0·10	0·06
Sodium		0 41		0·37		0·39		0·30		0·19	0·14
Potassium		..		..		trace		trace			
Sulphur		0·07		trace		minute trace		trace		trace	trace
Phosphorus		0·54		lost		0·07		0·40		0·12	0·37
		100·30		101·16		100·90		101·11		98·55	100·00

* The figures 3·71 in No. IV. (cold blast), as well as the corresponding figures in No. V. (cold blast), and Nos. I., III., IV. and V. (hot blast), indicate the per-centage of substance separated and weighed on filter, consisting, with the exception of No. V. (hot blast), principally of carbon and small quantities of silica, oxide of iron, lime, &c., and which I regret time did not permit me to make an accurate quantitative examination of. The figures on the left, marked C. indicate the *entire* amount of carbon in the iron.

The white iron can, on the other hand, be brought to assume the aspect and fracture of the gray iron by being frequently heated to whiteness and very gradually cooled down. It would seem, in fact, as though the carbon, in part at least, were held in solution in the molten iron, just as certain salts are in hot water; and that in the gray iron, during the process of cooling, a considerable part had been deposited in an uncombined state, either from its being present in larger quantity or from being more slowly cooled.

Difference in the per-centage of Phosphorus in the Hot and Cold Blast Iron.

	I.*	II.	III.	IV.	V.	VI.	VII.	VIII.
Cold blast....	0·47	0·41	0·31	0·20	0·21	0·36	0·03	0·36
Hot blast	0·51	0·55	0·50	0·71	0·54	..	0·07	0·40

Analysis of Compound separated from the Irons C. I., C. III. *and* H. VIII. *by Hydrochloric Acid.*

	C. I.		C. III
Carbon	32·36	Carbon	34·51
Silica..................	40·00	Silica	22·19
Peroxide of iron	19·00	Peroxide of iron	37·50
Traces of alumina, lime, &c.		Traces of lime, &c.	
Water and loss..........	8·64	Water	4·70
	100·00		98·90

	C. VIII.
Carbon	11·76
Iron..	79·52
Silica, with small quantities of oxide of iron, lime and alumina	9·48
	100·76

The carbon was determined by combustion with chromate of lead; the silica, &c. by fusion of the substance with a mixture of nitrate of potash and carbonate of soda. The 9·48 in C. VIII. was what remained on treating the fused mass with acid, evaporating to dryness, &c., and was composed as indicated. It will easily be seen that all, or *nearly* all, the iron in C. I. and C. III. must have been in the state of peroxide, whilst that of C. VIII. must necessarily have existed in the metallic state, or rather as a carburet. With the exception of a minute portion contained as a silicate, $Fe^3 C^2$ would require 73·00 iron for 11·76 carbon. When this substance was ignited for more than an hour over an Argand lamp, it was apparently little altered, and was nearly the same in weight.

The specimens of iron and iron ore were from the Level Iron Works near Dudley, belonging to Lord Ward, and were furnished by his agent, Richard Smith, Esq. of the Priory, to whom, as well as to his son, Mr. Frederick Smith, I am greatly indebted for the

* The first four numbers are taken from the former series before named; those from V. to VIII. from the present series. They are not numbered as in the Table of Analyses, but it will easily be seen to which they refer.

trouble and expense they have incurred at different times in forwarding this investigation. And I may be excused observing, in conclusion, that if the iron masters as a body exhibited the same degree of interest in the improvement of their manufacture, there would be such changes introduced as would prove of great national benefit; but at present, and so with this generation it will remain, *quantity* is the object in manufacture; *quality* is altogether beside the mark.

Laboratory, Temple Buildings, Birmingham.

From the Chemical Gazette for December 15, 1849.

ON THE PERIODICAL APPEARANCE OF SHOOTING STARS FROM THE 13TH TO THE 15TH OF NOVEMBER. BY VON HUMBOLDT *.

I learned with astonishment from the newspapers that it had just been stated to the Academy that the fall of shooting stars from the 12–14 November had advanced this year twenty-four days; that the fall of the Asteroids took place from the 15th to the 17th of October. This change of the node (of the intersection of the ring of the Asteroids and of the orbit of the earth), a change so abrupt from one year to another (the phænomenon having been invariably attached to the 12–17 of November from 1799 up to 1848), seemed to me hardly probable. In fact, the fall which is asserted to have been observed the 15–17 of October 1849, did not cause the great phænomenon of November in this same year to disappear. At the Observatory of Breslau, M. de Boguslawski and a great number of young students who are acquainted with the constellations and know how to observe the time, were placed at six large windows taking in the whole horizon. On the 12th of November, from $10^h\ 30^m$ to $12^h\ 30^m$, there were reckoned in all 88 shooting stars, 78 of which have been traced on maps: 1 was of the magnitude of Venus, 1 of Jupiter, 15 of stars of the first magnitude, 31 of the second magnitude. On the 13th of November, again twenty-six observers, but a little fog. They could only take observations from $10^h\ 30^m$ to $12^h\ 15^m$. They saw 69 shooting stars, 62 of which were marked on maps of the heavens. At $10^h\ 23'\ 12''$, mean time of Breslau, a fire-ball traversed from the Cameleopard toward the Great Bear. Also 1 shooting star as large as Venus, 9 as stars of the first magnitude, 20 of the second magnitude, 25 of the third magnitude. Observations will be made at Breslau from $10^h\ 30^m$ to $12^h\ 30^m$, from the 6th to the 12th of December, on account of the period on which I have insisted in the Kosmos. You will recollect that the three great falls of shooting stars (such as have not been seen in Europe in this century) were, on the 12–13 November 1799, Cumana; the 12–13 November 1833, North America; the 13–14 November 1834, North America. Since then the falls have frequently been

* Extract from a Letter to M. Arago. *Comptes Rendus,* Nov. 26, 1849.

in the night of 13–14 November, and we might have conjectured a movement of the node. You will remark that in 1849 the phænomenon has been most developed again the 12–13, such as M. Bonpland and I observed it half a century ago. The October stream, which has been observed this year, from 15–17 October, is independent of the November stream, since they have both been seen in the same year 1849. It is remarkable that the Arabian writers also notice two enormous falls, one of the 19 October 1202 (Fraehn, in the *Bulletin de l'Académie de Saint Pétersbourg*, t. iii. p. 308), and the other, October 902, on the night of the decease of King Ibrahim-ben-Ahmed (Conde, *Historia de la dominacion de los Arabes*, p. 346.). M. Sédillot might find the precise date of the death of this king. I think that many apparent anomalies are explained, if we admit that the stream is of a certain magnitude, a variable magnitude; and that the asteroids, in the annular zone, are unequally distanced and agglomerated. Have we not seen the comet of Biela divide into two comets, since December 19, 1845, each having its tail, advancing parallel at twenty minutes distance from one another! Cosmical nebulæ, that have so little *mass*, such as comets, fire-balls, and shooting stars, must be subject to undergo many transformations in form, direction and velocity.

ON TEREBIC ACID. BY M. A. CAILLOT.

M. A. Caillot, Professor of Medicine at Strasburgh, has obtained a new product which he calls terebic acid, by treating oil of turpentine with nitric acid.

This acid, under the influence of heat, as also all other acids, the temperature of which may be raised from 266° to 275° F., is converted into carbonic acid and pyro-terebic acid.

The formula of this acid uncombined is $C^{14}H^{10}O^{8}$: it produces several kinds of salts.

A. *Monobasic salts*: the formula of these is $C^{16}H^{9}O^{7}, MO$; they are soluble in water and crystallizable. They are obtained by treating terebic acid with insoluble carbonates, or by double decomposition.

B. Terebic acid forms other saline compounds which contain 2 equivalents of base for the 14 equivalents of carbon contained in the acid. If the circumstances resembled common bibasic acids, the formula of these salts would be $C^{14}H^{8}O^{6}, 2MO$. M. Caillot obtained only one thus constituted; it was an insoluble salt of lead.

In all the other bibasic salts, the water of the terebic acid, instead of being eliminated, remains in the salts, or even with certain bases, three equivalents are added. This water is essential to the constitution of the salt, for it cannot be expelled without disengaging pyrogenous compounds at the same time.

Adopting the generally received opinions of science, we are led to admit the existence of two new acids, both of them bibasic; one of these, which the author calls *diaterebic acid*, is represented by the formula $C^{14}H^{10}O^{8}+2HO$; the other, which he names *metaterebic acid*, has the formula $C^{14}H^{13}O^{11}+2HO$.

The formula of the metaterebates is $C^{14}H^{13}O^{11}$, 2MO; those obtained by the author are the metaterebates of barytes, strontia, potash, soda, magnesia and manganese. They are perfectly soluble in water, and most of them crystallizable. They are formed, either by neutralizing the acid with the bases, or by double decomposition. The metaterebates of zinc, nickel and iron, obtained by double decomposition, are almost immediately converted into monobasic terebates, and deposit half their bases.

The formula of the diaterebates is $C^{14}H^{10}O^{8}$, 2MO. M. Caillot has obtained the salts of lime, silver, copper and lead. All may be prepared by double decomposition, but the diaterebate of lime may be obtained by direct means. This last salt is soluble and crystallizable; the others are nearly insoluble.

C. *Compounds of Lead.*—Besides the terebate of lead and the diaterebate, M. Caillot has obtained several other remarkable compounds. By dissolving two equivalents of oxide of lead in an equivalent of terebic acid, there is composed a salt $C^{14}H^{8}O^{6}$ PbO, 2HO, soluble in water in all proportions. This compound can part with two equivalents of water and leave $C^{14}H^{8}O^{5}$ 2PbO, 2PbO.

On dissolving hydrate of lead in terebic acid, or still better, in a concentrated solution of monoterebate of lead, a compound is obtained which is soluble in water in all proportions, $C^{14}H^{10}O^{8}$ 5PbO $=$ $C^{14}H^{8}O^{6}$ 5PbO, 2HO. By long-continued ebullition the solution deposits a new salt, the formula of which is $C^{14}H^{8}O^{6}$ 5PbO, 2PbO. A cold solution of metaterebate of barytes dissolves a large quantity of the oxides of lead and silver. New compounds are formed, respecting which M. Caillot has promised future communications. From the facts contained in this memoir it may be concluded—

1st. That terebic acid combines with all bases so as to produce monobasic salts.

2nd. That when there is an excess of an alkaline base, this acid combines with various proportions of water according to the nature of the bases, to constitute two new acids, namely, the diaterebic and metaterebic, both of which are bibasic.

3rd. That oxide of lead forms with the terebic acid several compounds, among which are two that are without analogy in chemistry, and which the author is inclined to consider as performing the functions of acids.

4th. That the combination of water in the diaterebates and the metaterebates appears to be the essential condition of the bibasity of these salts. The author is of opinion that this proposition acquires additional probability, on considering the facts observed respecting the phosphates by Mr. Graham, and those respecting the phosphites and hypophosphites by M. Wurtz, and the acids of antimony and tin by M. Fremy.—*L'Institut*, Novembre 7, 1849.

ON THE ÆTHEREAL COMPOUNDS OF TEREBIC ACID.

BY M. A. CAILLOT.

The results obtained by the author with respect to the composition of the terebates, induced him to direct his researches to the æthereal

compounds of terebic acid. Hitherto he has obtained only the compounds which correspond to the monobasic terebates; they are represented by the general formula $C^{14}H^{9}O^{7}(C^{m}H^{n})O = C^{14}H^{8}O^{6}$, HO, $(C^{m}H^{n})O$. He has obtained the vinic, amylic and methylic compounds, which in their general physical properties much resemble others; thus at common temperatures they are liquid, oleaginous, slightly soluble in water; they have a hot, acrid, bitter, but not an acid taste; the vinic and amylic compounds may be distilled without undergoing decomposition. M. Caillot was able to determine the density of the vapour of the vinic compound: it confirmed the results of the elementary analyses. The methylic compound is altered by distillation.

These compounds act with bases like acids, but the salts which they produce are not permanent. The terebovinate proper for analysis was obtained with great difficulty; its formula is $C^{14}H^{8}O^{6}$, BaO, $C^{4}H^{5}O$. With the slightest increase of temperature these vinic salts reproduce the alcohol from which they are formed, and generate a bibasic metaterebate, half of the vinic acid being set free. This transformation may be represented by the following equation:

$$2(C^{44}H^{8}O^{6}, BaO, C^{4}H^{5}O) + 7HO, (C^{14}H^{13}O^{11}\,2BaO) + C^{4}H^{6}O^{2} + (C^{14}H^{8}O^{6}, HO, C^{4}H^{5}O).$$

The property which terebic acid possesses of forming vinic acid, seems to indicate that this acid may produce bibasic anhydrous salts, of the formula $C^{44}H^{8}O^{6}\,2MO$.—*L'Institut*, Novembre 7, 1849.

PARATARTARIC ACID.

M. Kestner states that this acid was produced for some years, or from 1822 to 1824 nearly. At this time the tartrates were saturated with carbonate of lime, and the remainder of the tartrates were precipitated by chloride of calcium; the tartrate of lime was decomposed by great excess of sulphuric acid, evaporation was effected over the naked fire, and the solutions of tartaric acid were decolorized by a current of chlorine gas; this operation was performed cold, and it was then remarked, and more especially in winter, that crystals of paratartaric acid were formed which were carefully separated, because they rendered the crystallization of the tartaric acid irregular when they remained mixed with it.

M. Kestner states that since the above-stated period, tartar has been saturated by caustic lime, and the tartrate of lime formed is decomposed by a slight excess of sulphuric acid; no chlorine is now employed to decolorize the liquors, and no trace of parataric acid is now obtained.

It is stated by M. Kestner that he is entirely ignorant whether the circumstances which he has stated have produced the paratartaric acid: he has never been able to produce it, neither by the agency of sulphuric acid, even at high temperatures and heating them for a long time together, nor by the action of chlorine. Nor has he succeeded in extracting it directly from tartar, although the attempt has been made on considerable quantities.

M. Kestner states that he is informed that Mr. Whyte, a manufacturer of tartaric acid at Glasgow, has also produced paratartaric acid. He regrets that he is unable to say more on the subject, and shall be happy if science should put him in the way of reproducing this interesting acid.—*L'Institut*, Novembre 14, 1849.

METEOROLOGICAL OBSERVATIONS FOR NOV. 1849.

Chiswick.—November 1. Fine. 2. Foggy: very fine. 3. Dense fog. 4. Foggy: cloudy at night. 5. Cloudless and fine. 6. Clear. 7. Rain. 8. Densely overcast. 9. Overcast: fine: overcast. 10. Exceedingly fine. 11. Clear and fine: foggy at night. 12. Foggy: hazy. 13. Fine: rain. 14. Heavy rain: clear at night. 15. Clear: cloudy: clear. 16. Fine. 17. Clear. 18. Overcast: slight rain: showery at night. 19. Hazy. 20. Uniformly overcast. 21. Hazy. 22. Foggy: overcast. 23. Overcast: rain. 24. Foggy. 25. Foggy: cloudy: clear. 26. Foggy: cloudy and cold: clear: sharp frost at night. 27. Sharp frost: clear: foggy. 28. Frosty and foggy: clear and frosty: foggy. 29. Overcast: fine: overcast. 30. Constant rain.

Mean temperature of the month	41°·99
Mean temperature of Nov. 1848	41 ·18
Mean temperature of Nov. for the last twenty-three years	43 ·41
Average amount of rain in November	2·56 inches.

Boston.—Nov. 1. Cloudy. 2. Fine. 3, 4. Foggy. 5, 6. Fine. 7. Cloudy: rain A.M. and P.M. 8—10. Cloudy. 11. Fine. 12. Foggy. 13—17. Fine. 18. Cloudy: rain A.M. 19, 20. Fine. 21, 22. Cloudy. 23. Cloudy: rain P.M. 24—26. Fine. 27. Fine: snow A.M. 28. Snow. 29. Cloudy. 30. Rain: rain A.M.

The following are the averages for Oct. 1849, with which we have been favoured by our correspondent Mr. W. Veall of Boston, whose report did not arrive in time for our last Number.

Barometer.	Thermometer.	Rain in inches.
29·46	48·8	3·32

Applegarth Manse, Dumfries-shire.—Nov. 1. Rain during night: cleared P.M. 2. Raw frost A.M.: rain: fog P.M. 3. Raw frost again: threatening rain. 4. Fine A.M.: rain and high wind P.M. 5. Heavy showers all day. 6. Hard frost A.M.: storm of snow P.M. 7. Frost: snow nearly all day. 8—10. Slight drizzle: damp all day. 11. Heavy rain and thick. 12. Dull A.M.: fine noon: wet P.M. 13. Heavy showers, with blasts. 14. Clear and cold, with showers. 15, 16. Frost: clear and fine. 17. Close rain and mist all day. 18. Rain during night: mild: rain. 19. Rain during night: cleared: warm. 20. Rain and fog all day. 21. Fair, but dull. 22. Rain and fog throughout. 23. Rain and fog: cleared P.M. 24. Frost, hard: grew mild. 25. Frost not so hard: rain: fog. 26. Frost hard again. 27. Frost hard all day. 28. Frost very hard. 29. Snow: hard frost: rain P.M. 30. Thick fog: heavy rain: cleared.

Mean temperature of the month	42°·0
Mean temperature of Nov. 1848	39 ·8
Mean temperature of Nov. for the last twenty-five years ...	40 ·4
Mean rain in November for twenty years	3·60 inches.

Sandwick Manse, Orkney.—Nov. 1. Fine: large halo: aurora. 2. Showers. 3. Fine: showers. 4. Cloudy: showers. 5. Showers. 6, 7. Snow: snow-showers. 8. Snow-showers: rain. 9. Cloudy. 10. Showers. 11. Bright: cloudy: aurora. 12. Bright: clear: aurora. 13. Showers: aurora. 14. Bright: clear: aurora. 15. Bright: frost: showers. 16. Clear: frost: clear. 17. Showers: rain. 18. Drizzle: clear. 19. Fine: clear: aurora. 20. Drizzle: damp. 21. Cloudy: damp. 22. Fine: clear. 23. Rain. 24. Fine: clear: aurora. 25. Fine: frost: clear. 26. Rain. 27. Clear. 28. Clear: frost: cloudy. 29. Bright: showers. 30. Fine.

Meteorological Observations made by Mr. Thompson *at the Garden of the Horticultural Society at* Chiswick, *near London; by* Mr. Veall, *at* Boston; *by the* Rev. W. Dunbar, *at Applegarth Manse,* Dumfries-shire; *and by the* Rev. C. Clouston, *at Sandwick Manse,* Orkney.

Days of Month.	Barometer.							Thermometer.							Wind.				Rain.			
	Chiswick.		Boston. 8¼ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick.		Boston. 8¼ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.
1849. Nov.	Max.	Min.		9 a.m.	9 p.m.	9¾ a.m.	8¼ p.m.	Max.	Min.		Max.	Min.	9¾ a.m.	8¼ p.m.								
1.	29·605	29·439	29·13	29·37	29·51	29·64	29·67	57	35	47	51	42	46	42	se.	s.	ene.	calm	......	......	......	......
2.	29·607	29·578	29·29	29·50	29·49	29·53	29·45	58	37	47	50	36	44½	45	e.	s.	sw.	sw.	......	......	......	·10
3.	29·503	29·345	29·25	29·43	29·25	29·43	29·19	55	42	38	50½	36	46	44	e.	w.	se.	s.	......	......	......	·33
4.	29·234	29·055	28·86	29·05	28·70	28·96	28·67	54	36	45	50	41½	46½	42½	e.	sw.	s.	calm	·02	......	......	......
5.	29·308	29·106	28·65	28·63	28·98	28·53	28·77	53	33	38·5	50½	36	42	35	sw.	ssw.	sw.	wnw.	......	......	......	·29
6.	29·851	29·518	29·16	29·26	29·50	29·18	29·47	50	31	36	48	29½	35	35	sw.	sw.	sw.	nw.	......	......	......	·27
☾ 7.	30·077	29·973	29·69	29·62	29·70	29·69	29·82	57	40	36	36½	32	36	34	sw.	s.	ese.	s.	·05	·27	......	·19
8.	30·313	30·254	29·80	29·87	29·90	29·90	29·67	59	52	53	54	35½	36½	51	sw.	ssw.	s.	ssw.	......	......	0·80	·22
9.	30·330	30·253	29·86	29·96	29·94	29·82	29·90	60	44	53	54	51	52	51	sw.	ssw.	s.	wsw.	......	......	......	·41
10.	30·246	30·201	29·80	29·96	29·88	29·91	29·78	59	32	48·5	54	50	47½	51	se.	ssw.	s.	sse.	......	......	......	·03
11.	30·277	30·260	29·79	29·93	29·88	29·83	29·83	60	35	48	54	51	50	47	sw.	s.	sw.	s.	......	......	......	·06
12.	30·168	30·012	29·72	29·81	29·70	29·71	29·66	55	35	40·5	55	50	50	46	sw.	s.	sw.	ssw.	......	......	......	......
13.	29·896	29·708	29·90	29·52	29·50	29·44	29·40	55	39	33	53	46	47	45	sw.	calm	sw.	wsw.	·18	......	......	·08
● 14.	29·682	29·576	29·79	29·32	29·35	29·36	29·36	55	36	40	46	39	41	47	sw.	calm	sw.	calm	·19	......	......	......
15.	29·720	29·604	29·72	29·42	29·73	29·53	29·89	48	35	39	46	34½	41½	42	sw.	nw.	w.	n.	·08	......	......	......
16.	30·087	29·967	29·64	29·92	30·05	30·03	30·13	48	29	39·5	46	36	38	35½	nw.	n.	ne.	e.	......	......	0·66	·24
17.	30·241	30·209	29·88	30·02	30·00	30·03	29·77	46	25	32	47	40	41½	48	nw.	nnw.	sw.	ssw.	......	......	......	·10
18.	30·211	29·987	29·72	29·80	29·80	29·62	29·90	52	43	42	52½	43½	50½	45½	sw.	s.	sw.	nw.	·05	·05	......	·57
19.	30·068	29·970	29·64	29·90	29·94	29·99	30·05	52	31	42·5	50½	40	43½	42	ne.	nnw.	w.	ese.	......	......	......	·02
20.	30·109	30·100	29·75	29·92	29·93	30·04	30·00	49	41	43	50½	45½	45½	47½	e.	s.	sw.	se.	......	......	......	......
21.	30·074	30·015	29·70	29·87	29·81	29·93	29·90	48	34	44	49	44½	48	47½	e.	s.	ese.	se.	......	......	......	......
22.	29·896	29·749	29·55	29·69	29·53	29·72	29·50	45	32	38	47	43	45½	47	se.	s.	e.	w.	......	......	......	......
☽ 23.	29·677	29·326	29·30	29·29	29·18	29·34	29·27	52	37	39	47½	33½	45½	42	s.	s.	se-sw.	ne.	·36	......	0·63	·11
24.	29·361	29·303	29·01	29·20	29·18	29·30	29·26	41	28	33	45	29	40	36½	se.	w.	se.	calm	......	·32	......	·89
25.	29·552	29·356	29·12	29·46	29·58	29·35	29·59	42	27	32	40½	30½	44½	38	nw.	w.	se.	se.	......	......	......	......
26.	30·065	29·732	29·54	29·73	29·95	29·88	30·07	38	18	35	39	28	40½	39½	ne.	ese.	se.	sse.	......	......	......	·37
27.	30·138	30·136	29·87	30·04	30·03	30·12	30·12	36	21	30	36	28½	38½	38½	nw.	wnw.	e.	se.	......	......	......	·07
28.	30·051	29·940	29·74	29·90	29·72	29·97	29·84	33	21	29	33	21	37	37	w.	nnw.	sse.	se.	......	......	......	......
29.	29·973	29·932	29·65	29·60	29·42	29·65	29·37	45	31	32	41	24½	36	41	s.	s.	s.	s.	·16	......	......	......
○ 30.	29·894	29·786	29·50	29·57	29·69	29·52	29·67	48	28	38	44½	39½	41	41½	sw.	s.	e–sw.	s.	·23	·10	0·60	·10
Mean.	29·940	29·779	29·53	29·618	29·627	29·631	29·632	50·33	33·60	39·7	47·3	37·9	43·23	42·82					1·32	0·74	2·69	4·45

THE

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JOURNAL OF SCIENCE.

[THIRD SERIES.]

FEBRUARY 1850.

IX. *On the Electromotive Force of Gases.* *By* W. Beetz, Ph.D.*

INVESTIGATIONS upon the measurement of the electromotive force of gas batteries are still wanting. The only experiments which aim at quantitative determination, are those in which Grove† measured the change in the volumes of the gases contained in the tubes of the battery. As a rule, the diminution of the volumes was found greater in proportion to the activity of the current produced by the gas. If, however, we require to draw any conclusion regarding electrical excitation from this measurement, the hypothesis of Grove, viz. that the combination of the two gases used is the source of the electricity, and hence the circuit produces no effect when the gases have no chemical affinity for each other, must first be proved. Even then much deception might occur in consequence of secondary actions, such as were observed by Grove himself, so that in this manner we should never be able to obtain values which could in any way be regarded as measures of the electromotive force of gas batteries. Moreover, in consequence of the great inconstancy of gas batteries, at least with the use of most gases, experiments made with batteries which have remained closed for a long time consecutively, only give very dubious results, because with many of them the polarization entirely renders insensible the original electromotive force. Even in the case of those which are apparently the most constant, those where hydrogen and oxygen, and hydrogen and chlorine are used, the original force is quickly diminished; the polarization cannot however here attain any very considerable value, because the gases set free by

* From Poggendorff's *Annalen*, vol. lxxvii. p. 493.

† Phil. Trans. 1843, part. 2. p. 91. [See also Phil. Mag. vol. xxiv. p. 268.]

electrolysis are for the most part again removed, and thus after some time great constancy obtains.

The object of the following investigations is therefore to determine the electromotive force of the gas-battery quite independently of, or as much so as possible, the disturbing influence which prolonged closure of the current must exert. For this purpose, the method of compensation proposed by Poggendorff* naturally presented itself; in the application of which, the resistance of the circuit, the measurement of which is desired, is entirely unknown, and the circuit requires to be closed for a moment only. If three conductors, the resistances of which are r, r' and r'', meet in two points, r being a circuit the electromotive force of which is k', and r' one the force of which is k'', the total intensity J'' existing in r'' will be equal to the intensity which k'' would produce alone, *minus* that which k' would have produced by its own action; hence

$$J'' = \frac{k''}{r'' + \frac{rr'}{r+r'}} - \frac{k'}{r' + \frac{rr''}{r+r''}} \cdot \frac{r}{r+r''}.$$

When $J''=o$, we have

$$o = k''(r+r') - k'r$$

$$k'' = \frac{r}{r+r'} \cdot k'.$$

Hence when the magnitude k' is given, k'' is also known by the measurement of r and r'. The resistances were measured by a rheochord spun over with German silver wire, in the same manner as that adopted by Poggendorff; r is represented directly by a length of wire b; r' consists of a length of wire a, the resistance of the circuit k' (a Grove's platino-zinc circuit), $=w$, and the resistance of a galvanometer, with a simple needle and but few coils, inserted in this closing wire, $=g$. Lastly, the resistance r'' consists of that of the circuit measured, and that of a delicate galvanometer G provided with an astatic arrangement, with its conducting wires. Let the resistances $w+g$ be $=R$. After the positive plate of the circuit under measurement (*ex. gr.* that platinum plate which is coated with hydrogen) had been connected by the wire b with the platinum plate of the Grove's circuit, by the wire a and the galvanometer g with the zinc plate of the latter, a wire from the negative plate of the circuit under measurement (platinum coated with oxygen) was connected with a delicate galvanometer. Moreover, a wire from the platinum plate of

* Poggendorff's *Annalen*, vol. liv. p. 180.

the Grove's circuit was placed in a cup of mercury, so that the current in r'' could be closed by rapidly immersing the second wire of the galvanometer in this cup. The length of b was then fixed by the running clamp-screw of the instrument for measuring the resistance, and a altered until the needle of the galvanometer G on closing the circuit r'' remained at 0. We then have

$$k'' = \frac{b}{a+\mathrm{R}+b}k'.$$

To find R, b then obtained another value b'; a then became a', so that

$$k'' = \frac{b'}{a'+\mathrm{R}+b}k'.$$

R was calculated from both equations. In the present instance this method is very convenient, and sufficiently exact also, when the circuit to be measured is not too inconstant (as that in which hydrogen and oxygen are used). The following series of experiments show that this point was attended to. By a controlling measurement made by Ohm's method, I convinced myself of the applicability of the above process. Lastly, to ascertain the force k', the measuring platino-zinc circuit was closed by the wire of the rheochord in which a had been previously measured, the needle of the galvanometer was then driven to 24°, which deflection I had found to correspond to an evolution of 13·36 cubic centimetres on an average of the mixed gases in the proportion for forming water per minute. When the necessary resistance was ρ, we had

$$k' = \mathrm{R} + \rho,$$

where unity is that electromotive force, which, with a resistance of one centimetre German silver wire of 8·689 specific gravity (the average of four weighings), a centimetre of which weighs 0·00683 grm. (the mean of five determinations), evolves 13·36 cubic centimetres of the mixed gases per minute. With this value of unity, the force of the platino-zinc circuit is about =42. From these determinations the values found may be readily compared with any others. If, for example, we convert the measurements made by M. Poggendorff from his unit, in which the force of the Grove's circuit is about =32, into mine, this force becomes 42·27. A measurement may be adduced as an example of this.

Series.	Observation.	Positive.	Negative.	a.	b.	R.	ρ.	k''.
1	1	H	O	69·6	120	19·2	22·6	24·02
...	2	...	...	99·2	160			

in which

$$k'' = \frac{b}{a+\mathrm{R}+b}(\mathrm{R}+\rho).$$

The value found for R in both observations was assumed to be constant during the whole series of observations, which never occupied a period of more than two hours; the same applies to ρ. All those connexions which were not (like r'') of equal value, or had been ascertained by calculation from their resistance (as those contained in R), were effected by short pieces of copper wire of the diameter of about a line.

Before detailing the experiments in full, a few words more must be said upon the preparation of the gas batteries. To obtain the platinized platinum in its most effective state, the plan proposed by M. Poggendorff was followed; the platinization was effected in a dilute solution of chloride of platinum by a current from two of Grove's cells, and thus a black coating was produced. It will be subsequently seen, however, that in the method of measuring adopted, a gray, imperfectly acting precipitate cannot produce any great changes in the force of the circuit, as the activity of the finely-divided layer is altogether only a secondary one. If two platinum plates are used to a gas battery, care must first be taken that they are not capable of exciting any voltaic tension of themselves; they must be tested as to their being perfectly homogeneous; and if they are not, they must be made so. Serious error may be fallen into in regard to this point, if the ordinary means are used for connecting the plates with each other in the conducting fluid for any time. If, for instance, upon one of the plates hydrogen in addition to the platinum is precipitated by a somewhat strong current, as is usually the case, and this is combined with a clean plate of platinum to form the circuit, hydrogen separates upon the latter plate; hence the current will cease before the oxygen evolved at the former plate has removed the hydrogen which it met with; consequently the plates appear to be homogeneous, but are far from consisting of pure platinum. I always took care that the hydrogen was first entirely dissipated, by causing all the plates about to be used in a series of experiments to act as the anodes for a short period. The oxygen and the chlorine by which they were then coated are much more easily removed, either by connecting all the plates, on the one hand, with another plate of platinum in a concentrated solution of platinum, so as to form a circuit, or by boiling them for a long time with water, or what is still better, by subjecting them to both these processes in succession. The plates, which were about four inches in length

and a quarter of an inch in breadth, were therefore cemented into corks before platinization, and after the plates had been connected in the platinum solution, they were inserted into tubes about five inches in length; the tubes were then filled with water; this kept boiling for several minutes, and then replaced by the conducting fluid (dilute sulphuric acid of 1 per cent.). This was also heated to ebullition, and the open end of the tube then immersed in a vessel, which also contained some of the boiled conducting fluid. The gas under experiment, in its utmost state of purity, was then admitted into the tube in such a manner that about one-third of the plate dipped into the liquid. To connect two tubes so as to form a circuit, an inverted U-shaped tube, filled with the conducting liquid and connected at each end with a bladder, was dipped into the two vessels which contained the tubes; by this arrangement, diffusion of the gases through the liquid was better prevented than when both the tubes were placed in the same vessel. If, in measuring, the proportion of a to b in the first experiments was found to be too defective, so that the gas-circuit was traversed by a perceptible current, it was first again tried until the galvanometer G remained at 0; the gas battery was then taken to pieces, and the whole process of filling it commenced anew. In the second measurement we commence with test experiments, which are not so far removed from the truth as to be capable of altering the battery perceptibly. The results last obtained only are given. After a little practice, we are generally enabled to avoid making too great mistakes in the position of the binding-screws, particularly when we have obtained an insight into the law which is deducible from the experiments.

Series.	Observations.	Positive.	Negative.	a.	b.	R.	ϱ.	k''.
I.	1	H	O	69·6	120	19·2	22·6	24·02
	2	....	....	99·2	160			
	3	H	Pt	108·8	120	....	....	20·23
	4	Pt	O	200	20	....	....	3·49
II.	1	H	O	72	120	19·2	23	23·98
	2	....	....	102·4	160			
	3	Pt	O	212	20	....	....	3·36
	4	H	Pt	108	120	....	....	20·48
III.	1	H	O	71·2	120	16·8	25·2	24·23
	2	....	....	100	160			
	3	H	CO_2	93·6	120	....	....	21·88
	4	CO_2	O	350	20	....	....	2·12
	5	H	NO	101·2	120	....	....	21·18
	6	NO	O	240	20	....	....	3·03
	7	NO	CO_2	360	6	....	....	0·66
IV.*	1	H	O	61·8	120	20·4	20·2	24·10

* The tubes filled as in series III. were retained.

Table (*continued*).

Series.	Observations.	Positive.	Negative.	*a.*	*b.*	R.	*ϱ.*	*k''.*
IV.	2	....	....	89·2	160			
	3	H	NO	86·4	120	....	....	21·48
	4	H	CO	208	120	....	....	13·98
	5	CO	CO_2	132	40	....	....	8·44
	6	CO	O	100	40	....	....	9·88
	7	H	Cy	98·6	120	....	....	20·38
	8	Cy	O	208	20	....	....	3·27
	9	CO	NO	160	40	....	....	7·37
V.	1	H	CO	116	60	26·0	14·8	12·40
	2	H	O	58	120	....	....	24·03
	3	....	....	86	160			
	4	H	Cy	93	120	....	....	21·10
	5	CO	Cy	226	60	....	....	7·84
	6	CO	O	134	60	....	....	11·13
	7*	H	CH_2	150	120	....	....	16·54
	8	CH_2	O	186	40	....	....	6·48
	9	CO	CH_2	292	30	....	....	3·52
VI.	1	H	Air	101·0	120	17·8	22·8	20·50
	2	....	....	140·6	160			
	3	H	Cl	38	160	....	....	30·25
	4	Air	Cl	180	60	....	....	9·50
	5	CO	Cl	120	100	....	....	17·15
	6†	H	NO_2	140	100	....	....	15·83
	7	NO_2	Cl	138	80	....	....	13·84
	8	H	CO	122	60	....	....	12·25
VII.	1	H	O	85·6	120	15·8	26·6	22·99
	2	....	....	119·4	160			
	3	H	NO_2	116	120	....	....	20·22
	4	H	CO	197	100	....	....	13·56
	5	H	Cl	35·2	160	....	....	32·27
	6	NO_2	O	300	20	....	....	2·52
	7	Cy	O	360	8	....	....	0·88
	8	H	Cy	95·6	120	....	....	21·99
	9	NO_2	Cy	366	14	....	....	1·50
	10	CO	O	123	40	....	....	9·48
	11	CO	NO_2	198	40	....	....	6·68
	12	CO	Cy	140·6	40	....	....	8·63
	13	O	Cl	220	60	....	....	8·60
	14	NO_2	Cl	139	60	....	....	11·84
	15	Cy	Cl	150	60	....	....	11·26
	16	CO	Cl	92·8	100	....	....	20·32
VIII.	1	H	O	69·6	120	19·8	21·0	23·38
	2	....	....	99·4	160			
	3‡	H	CO	224	40	....	....	5·15
	4	CO	O	149	120	....	....	16·95
	5	H	Cl	29	160	....	....	31·41
	6	CO	Cl	53	120	....	....	25·39
IX.	1	H	O	68	120	17·2	24·2	24·21
	2	....	....	96·4	160			
	3§	H	CO	230	40	....	....	5·76

* The CH_2 gave off an odour of æther.

† The NO_2 probably contained hydrogen.

‡ The CO probably contained hydrogen.

§ The CO had been used previously, the other gases were freshly prepared.

Table (*continued*).

Series.	Observations.	Positive.	Negative.	*a.*	*b.*	R.	ϱ.	*k''.*
IX.	4	H	CH_2	112	120	....	....	19·94
	5	H	Cl	29·6	160	....	....	32·03
	6	H	HS	360	30	....	....	3·05
	7	CO	O	128	100	....	....	16·88
	8	CO	CH_2	112	60	....	....	13·13
	9	CH_2	O	344	30	....	....	3·17
	10	O	Cl	244	60	....	....	7·72
	11	CO	Cl	54	120	....	....	25·98
	12	CH_2	Cl	206	80	....	....	10·92
	13	HS	O	108	120	....	....	20·26
	14	HS	CO	276	30	....	....	3·84
	15	HS	Cl	58	160	....	....	28·16
	16	H	O	69	120	....	....	24·09
	17	H	Pt	115	120	....	....	19·68
X.	1	H	O	87·8	120	12·4	29·8	23·00
	2	....	....	121·2	160			
	3	H	Br	68	160	....	....	28·09
	4	O	Br	270	40	....	....	5·23
	5	H	CS_2	126	120	....	....	19·60
	6	CS_2	O	240	20	....	....	3·10
	7	H	CO_2	112·8	120	....	....	20·65
	8	CO_2	O	290	20	....	....	2·62
	9	CO_2	Br	172	40	....	....	7·52
	10	CS_2	Br	152	40	....	....	8·26
XI.	1	H	CO_2	122·2	120	9·8	33	20·38
	2	....	....	166·2	160			
	3	H	CO	142·0	60	....	....	12·12
	4	H	Br	72·6	160	....	....	28·32
	5	CO	Br	152	100	....	....	16·37
	6	CO_2	Br	266	60	....	....	7·65
	7	CO	CO_2	219	60	....	....	8·88
XII.	1	H	Br	73	160	10·2	31·6	27·50
	2	....	....	93·8	200			
	3	H	CO	114	60	....	....	13·61
	4	H	P	192	120	....	....	16·06
	5	CO	Br	188	100	....	....	14·02
	6	P	Br	200	80	....	....	11·52
XIII.	1	H	O	78·2	120	8·8	33·6	24·58
	2	....	....	107·2	160			
	3	H	NO_2	115·2	120	....	....	20·85
	4	NO_2	O	194	20	....	....	3·81
XIV.	1	H	O	79·6	120	6·8	36·0	24·88
	2	....	....	108·4	160			
	3	H	CH_2	149·4	120	....	....	18·59
	4	CH_2	O	109·4	20	....	....	6·28
XV.	1	H	O	78·4	120	9·2	32·8	24·27
	2	....	....	107·6	160			
	3	H	CO	121	60	....	....	13·25
	4	CO	O	145·4	60	....	....	11·74

It is distinctly evident from these series of experiments, that the electromotive forces of gases, connected by platinized platinum and dilute sulphuric acid so as to form gas batteries, follow the same law of Volta's tension series as the metals.

Sufficient agreement of the experiments with this law might be obtained if the means of the experiments made with the same matters were taken, and these were considered as the true electromotive forces; but, properly speaking, only those experiments belonging to one and the same series ought to be compared with each other; because in them, even an alteration in the composition of a gas, an impurity acquired by it during the filling of the tubes, &c. cannot exert any influence; those alterations only would become perceptible which the gas might have experienced in making the measurements themselves. In the following tables, the sums of the electromotive forces of different circuits are compared with the electromotive forces which were observed between the outermost members of these circuits.

Series.	Circuits.	Calculated.	Observed.
I.	H, Pt+Pt, O	23·72	24·02
II.	H, Pt+Pt, O	23·84	23·98
III.	H, CO_2+CO_2 O	24·00	24·23
IV.	H, NO+NO, O	24·21	24·10
	H, CO+CO, O	23·86	
	H, CO+CO, CO_2+CO_2, O	24·54	
	H, CO+CO, NO+NO, O	24·35	
	H, CO+CO, NO+NO, CO_2+CO_2, O	24·13	
	H, Cy+Cy O	23·65	
	H, NO+NO, CO_2+CO_2, O	23·96	
V.	H, CO+CO, Cy	20·24	21·10
	H, CO+CO, O	23·53	24·03
	H, CO+CO, CH_2+CH_2, O	22·40	
	H, CH_2+CH_2, O	23·02	
VI.	H, air + air, Cl	30·00	30·25
	H, CO+CO, Cl	29·40	
	H, NO_2+NO_2, Cl	29·67	
VII.	H, NO_2+NO_2, O	22·74	22·99
	H, CO+CO, O	23·04	
	H, Cy+Cy, O	22·87	
	H, CO+CO, NO_2	20·19	20·22
	H, CO+CO, Cy	22·19	21·99
	H, NO_2+NO_2, Cy	21·72	
	H, O+O, Cl	31·59	32·27
	H, CO+CO, Cl	33·88	
	H, NO_2+NO_2, Cl	32·06	
	H, Cy+Cy, Cl	33·25	
	H, CO+CO, NO_2+NO_2, Cy+Cy, O+O, Cl	31·22	
VIII.	H, CO+CO, O	22·10	23·38
	H, CO+CO, Cl	30·54	31·41
IX.	H, CO+CO, O	22·64	24·24
	H, CH_2+CH_2, O	23·11	24·09
	H, HS+HS, O	23·31	
	H, CO+CO, CH_2	18·89	19·94
	H, HS+HS, CO	6·89	5·76
	H, HS+HS, Cl	31·21	32·03
	H, CO+CO, Cl	31·74	
	H, CH_2+CH_2, Cl	30·96	

Table (*continued*).

Series.	Circuits.	Calculated.	Observed.
IX.	H, O+O, Cl	31·93	
	H, HS+HS, CO+CO, CH_2+CH_2, O+O, Cl	30·91	
X.	H, CS_2+CS_2, O	22·70	23·00
	H, CO_2+CO_2, O	23·27	
	H, O+O, Br	28·23	28·07
	H, CS_2+CS_2, Br	27·86	
	H, CO_2+CO_2, Br	28·17	
	H, CS_2+CS_2, O+O, Br	27·93	
XI.	H, CO+CO, CO_2	21·00	20·38
	H, CO+CO, Br	28·49	28·32
	H, CO_2+CO_2, Br	28·03	
	H, CO+CO, CO_2+CO_2, Br	28·65	
XII.	H, CO+CO, Br	27·63	27·50
	H, P+P, Br	27·58	
XIII.	H, NO_2+NO_2, O	24·66	24·58
XIV.	H, CH_2+CH_2, O	24·87	24·88
XV.	H, CO+CO, O	24·99	24·27

The agreement between the observed and calculated values is sufficient to place beyond doubt the position, that gas batteries also follow the electromotor law of Volta's tension series.

The electromotive forces given in the following table are the mean results of the observations. The platinum coated with hydrogen is taken as the starting-point, the numbers opposite the several substances indicate the electromotive force which they excite when placed in one of the gas tubes, hydrogen being placed in the other. To give a clearer idea of the absolute value of the forces, those of platinum and zinc and platinum and copper have also been determined, referred to the same starting-point, and added to the series. In determining the mean, the observations VI., 6; VIII., 3; and IX., 3, have been neglected for the reasons there stated. For the sake of comparison, I have placed the series found by Grove* opposite my own.

−31·49	Chlorine.	Chlorine.
27·97	Bromine.	Bromine.
		Iodine.
		Peroxides.
23·98	Oxygen.	Oxygen.
21·33	Nitrous oxide.	Nitric oxide.
21·16	Cyanogen.	
20·97	Carbonic acid.	Carbonic acid.
20·52	Nitric oxide.	Nitrogen.
20·50	Air.	

* Philosophical Transactions, 1845, p. 359.

20·13	*Platinum.*	*Metals* which do not decompose water.
19·60	Sulphuret of carbon.	Camphor, volatile oils.
18·36	Olefiant gas.	Olefiant gas.
		Æther, alcohol, sulphur.
16·06	Phosphorus.	Phosphorus.
13·02	Carbonic oxide gas.	Carbonic oxide gas.
3·82	*Copper.*	
3·05	Sulphuretted hydrogen.	
0	Hydrogen.	Hydrogen.
+19·68	*Zinc.*	*Metals* which decompose water.

The above results settle the question of dispute between Grove* and Schœnbein†, as to whether, in an oxyhydrogen gas battery, the hydrogen alone, or both gases are active. The opinion of the latter author is that the hydrogen alone produces the current; the presence of the oxygen would only then be of use in lessening the charge of the free plate of platinum produced by the current. Grove's experiments show that the presence of air is necessary for the continuance of the current; for when the gas battery remained enclosed under a globe filled with air, from which the oxygen was removed by the combustion of phosphorus, the intensity of the current sunk to 0, and on the admission of fresh air it gradually increased. This experiment, which corresponds perfectly with my experiments ‡ upon ordinary hydrogen batteries, Grove considers an argument against Schœnbein's view, and he is certainly correct. That the increase in the current caused by the oxygen is attributable to a diminution of the charge is indisputable; but it does not thence follow that the oxygen exerts no electromotive action. According to the experiments detailed above, the electromotive force of platinum and hydrogen is =20·13; of platinum and oxygen =3·85; the oxygen therefore contributes directly to the electric excitation, but certainly much less than the hydrogen. Air, nitrogen, and nitrogenous compounds act with oxygen still more feebly. The depolarizing action of the oxygen cannot be taken into consideration here, as the closure of the circuit is merely momentary.

As regards the situation of the electric excitation in the gas battery, this was found by Grove § to be at the point of contact of the platinum, gas and liquid. When the platinum did not

* Philosophical Transactions, 1843, p. 98. [Phil. Mag., vol. xxiv. p. 346.]
† Phil. Mag., vol. xxii. p. 165.
‡ Poggendorff's *Annalen*, vol. lxiv. p. 381.
§ Philosophical Transactions, 1843, p. 97. [Phil. Mag., vol. xxiv. p. 276.]

touch the liquid, no current was set up, as we should expect; when it was entirely covered by the liquid, the current was very feeble. Five oxyhydrogen pairs were not capable of decomposing iodide of potassium; but when the angles of the platinum were in contact with the gas, this decomposition was effected by only one pair of plates. This law does not, however, appear to me generally established. In the case of gases which are copiously absorbed by water, as chlorine, it is certainly incorrect*; in fact, I found with the circuit of hydrogen and chlorine that the electromotive force was always greatest when the chlorine contained in the tube was perfectly absorbed (free from air). It certainly holds good in the case of other gases, but in a less degree, especially when in ascertaining the intensity of the current, the circuit is closed for longer than a moment. The small quantities of gas dissolved by the conducting liquid are very rapidly consumed by the gas evolved electrolytically, and the liquid may not be able to absorb gas throughout its entire mass with sufficient quickness to bring the current again to a moderate intensity. The experiments of Jacobi and Poggendorff† tend to show that platinum also exerts an action upon the gas when covered by the liquid; they found that in a voltameter with platinized electrodes, the quantity of gas evolved entirely disappears again, even after the water has risen above it. I however avoided the use of a voltameter, at the poles of which the gases were evolved, because the electromotive force produced by the polarization is always greater than that of an ordinary gas battery. The plates of platinum were cemented into the tubes in the ordinary way, after the upper parts of them had been thickly coated with shell-lac. Only that part of the plates thus isolated was surrounded by the gases; those parts with a metallic surface remained completely immersed in the conducting liquid. In a circuit arranged thus, I obtained an electromotive force of 15·64, which is much too small. The principal cause of this was, that the conducting liquid, which at that heat would have absorbed too little gas, on cooling not only absorbed hydrogen from above, but also air from below, from contact with which it was not protected.

If the platinization of the plates of platinum only increases the electromotive force of gas batteries because the counter

* Mr. Grove did not mean to assert that there was no action when the electrodes were immersed in the solution and the gas soluble, as he treats of this action in other parts of his paper; but that even under these circumstances the points of electric action were those at which the platinum, liquid and gas met.—Ed. Phil. Mag.

† Poggendorff's *Annalen*, vol. lxx. p. 201.

charge is diminished, we ought to find, on applying the compensating method, that a circuit with platinized plates exhibited no more power than one with polished plates, provided the platinum had been purified sufficiently. I therefore constructed gas batteries of polished plates of platinum, which had been boiled in concentrated nitric acid and then in water. These batteries yielded the following results:—

Series.	Observations.	Positive.	Negative.	*a.*	*b.*	R.	*ρ*.	*k*″.
XVI.	1	H	O	72	120	14·4	27·0	24·07
	2	....	....	100·8	160	....	....	
	3	H	Cl	20	160	....	....	35·94
	4	H	Br	69	160	....	....	27·21
	5	O	Br	258	20	....	....	2·83
	6	O	Cl	154	60	....	....	10·87
	7	Br	Cl	184	40	....	....	6·94

This agreement was to have been anticipated from the experiments which Faraday* made upon the condensing action of pure platinum upon gases; and Poggendorff† has already rendered it probable, that the primary forces of gas batteries with polished, gray, or black platinized plates, would be the same in each case. But at the same time the agreement of these observations yields a proof, that with the extremely short closure of the circuit, which the compensating method requires, the polarization does not attain any important value; otherwise the polished plates, which are capable of being more powerfully charged, would give smaller values.

I finally passed to experiments with gas batteries, the solid conductor of which consisted of some other body than platinum. I then met with a great difficulty in the small values of the electromotive forces, which in most cases rendered a determination of the relation existing between them almost impossible. Even on using Bunsen's coke batteries, from which Poggendorff ‡ anticipated great action, the forces were not very great, and moreover still variable. It was only with very great difficulty that I could cut discs of the coke, even from the same piece, which were tolerably uniform. This is of more consequence, because after what has been stated above, we have no means of rendering them uniform artificially. The pieces of coke were boiled for several hours with nitric acid, then with water, afterwards with dilute sulphuric acid, and again with water; yet I very frequently found that when the tubes were filled with any inodorous gas in which these pieces

* Experimental Researches, § 570, 605.

† Poggendorff's *Annalen*, vol. lxi. p. 598.

‡ *Loc. cit.*

of coke were placed, in a short time they gave off an odour of sulphuretted hydrogen. Those experiments in which this occurred always yielded very small results, and were of course rejected. In those detailed below, no sources of error were remarked. They show that the relation of the electromotive forces, when different gases are used, is exactly the same as with platinum. I have therefore divided the mean electromotive force of an oxyhydrogen platinum battery by that of an oxyhydrogen carbon battery, and divided the forces of the other platinum batteries by the quotient *c*. In this manner the last column denoted by "calculated" was obtained. The resistances were determined by separate experiments.

Series.	Observations.	Positive.	Negative.	*a*.	*b*.	R.	*ϱ*.	*k*″. Observed.	*k*″. Calculated.
XVII.	1	H	O	208	60	12·4	28·6	11·16	11·24
XVIII.	1	H	O	97	40	13	28·2	10·99	11·24
	2	H	Cl	90	60	......	......	15·16	14·76
	3	O	Cl	160	20	......	......	4·27	3·52
	4	CO	Cl	192	60	......	......	9·33	8·65
XIX.	1	H	O	101	40	13	28·2	10·70	11·24
	2	H	Cl	80	60	......	......	16·15	14·76
	3	H	CO	220	40	......	......	6·04	6·10
	4	CO	O	280	40	......	......	4·84	5·14
	5	CO	Cl	160	60	......	......	10·60	8·65
XX.	1	H	O	94	40	14	27·2	11·14	11·24
	2	H	CO	160	40	......	......	6·75	6·10
	3	CO	O	200	20	......	......	4·45	5·14
	4	CO	Cl	180	40	......	......	7·07	8·65
	5	O	Cl	220	20	......	......	3·24	3·52
	6	H	Cl	98	60	......	......	14·37	14·76
	7	H	Br	122	60	......	......	12·61	13·11
	8	O	Br	350	20	......	......	2·90	1·87
	9	Br	Cl	300	10	......	......	1·27	1·65
XXI.	1	H	O	127	60	12·4	28·4	12·28	11·24

$$c=0{\cdot}4687.$$

The factor *c* evidently depends upon the condensation which the gases experience at the surface of the solid conductor, and by which the coating of the metal with the gases is rendered more perfect. Hence this method might be used for determining the relative condensing force of different bodies, if the absolute magnitudes of the electromotive forces in most matters were not too small. The coefficient 0·4687 cannot certainly be considered as applying to carbon generally. Other kinds of carbon may differ very greatly.

I obtained the following values in the case of gas batteries with chemically pure plates of silver, which were silvered galvanically:—

Series.	Observations.	Positive.	Negative.	*a.*	*b.*	R.	ϱ.	k''.	
								Observed.	Calculated.
XXI.	1	H	O	360	10	14·4	27·0	1·07	1·08
XXII.	1	H	O	366	10	9·2	32·8	1·09	1·08
	2	H	CO	350	4·6	......	......	0·54	0·58
	3	CO	O	360	4·4	......	......	0·49	0·50

$c = 0{\cdot}0449.$

Although the preceding experiments afford many explanations of the action of gas batteries, yet they cannot possibly decide upon the source of the force in them. The solution of this question, however, in my opinion, would constitute nothing less than a final decision as to the nature of electricity of contact. In fact, the phænomena which have just been discussed rank perfectly with those which occur in the excitation of a current by solid conductors, with the only difference, that the state of aggregation in the case of the gases may be altered by the solid bodies in contact with them, which is not the case with solid conductors: hence the difference in the absolute values in the case of the forces of the gas batteries, whilst their relative values remain the same. The special activity of the positive gases (as hydrogen) depends solely upon the electromotive force of platinum and hydrogen being far greater than that of platinum and oxygen, or even platinum and chlorine; the phænomenon observed by Matteucci* and others, that small quantities of hydrogen may exceed the action of large quantities of oxygen, then forms a simple analogy with the action of amalgams, in which the positive metal (potassium, zinc) preponderates.

Lastly, regarding the arrangement of the metals and gases in an electromotor series, as given above, we must not deceive ourselves regarding its importance. The metals mentioned (platinum, copper and zinc) hold that position to those gas batteries which contain plates of platinum; in the case of other batteries, their position would be different. Whilst, for instance, zinc is then positive towards hydrogen with a force of 19·68, Buff† found a zinc plate coated with platinum positive towards a polished plate, and subsequently gave a numerical value to this tension‡; from this he deduced the conclusion, that the position of hydrogen was nearer the positive end of a tension series than zinc, a conclusion which is evidently

* *Comptes Rendus*, xvi. p. 846.

† *Annal. der Chem. und Pharm.*, vol. xli. p. 136; *Archiv. de l'Electr.* ii. 222.

‡ Poggendorff's *Annalen*, vol. lxxiii. p. 505.

premature. He would only have been correct, provided, in the gas batteries, the metallic plates were so completely coated with the gases that they only acted as conductors, and did not come into immediate contact with the liquid. This is, however, evidently not the case; on the other hand, we always observe the simultaneous action of the metal forming the basis of the battery; a phænomenon which is evident when the gas does not come into contact with the plate itself, but is only slightly dissolved by the liquid; whilst, when the gas comes into immediate contact, *i. e.* at the point of contact of the metal, the gas and the liquid, the action of the gas is at its maximum, and the values then obtained are those given above. I cannot therefore entirely agree with one statement made by M. Buff*, viz. "that the same effect is produced by the layer of hydrogen at the negative plate of platinum (on polarization), as also by the layer of oxygen at the positive plate of platinum, as when, instead of two strips of platinum, a strip of solid hydrogen and a strip of solid oxygen had been introduced into the acid;" but I entirely agree with the following remark on this point:—"The electromotive action excited by the immediate contact of hydrogen and oxygen, or the electric difference of the two matters, denotes the extreme limit of the resistance (of the counter force) which can possibly be produced by the polarization of two metals in decomposing cells. This limit is more approximated in proportion to the power possessed by the immersed plates to become coated with the gases, and to the completeness with which the direct contact of the metallic with the liquid conductor is by this means avoided. If the partly immersed strips could be completely isolated from the liquid by the gases with which they become coated, the chemical nature of the metallic masses would be a matter of perfect indifference." But since the metals are not all equivalent when the polarization is at its maximum, it can never be admitted that the plates are completely coated with the gas, nor can the polarizing values be regarded as the true electromotive forces of the gases concerned.

Here, I think, is to be sought for the cause of the phænomenon, that plates of the same metal yield a far greater electromotive force when they are coated with the gases by polarization than when this is produced by any other means; they are coated much more perfectly by polarization. The electromotive force thus produced in M. Poggendorff's† experiments, when reduced to my unit, would be 55 for polished

* *Loc. cit* † Poggendorff's *Annalen*, vol. lxx. p. 179–189.

platinum, 40 for platinized platinum, whilst I found it = 24 with the gas battery.

I should wish on this point to recall the attention of the reader to the remarks of this philosopher* upon the predominating action of secondary batteries over that of the gas battery.

X. *The* Astronomer Royal *on a Problem of Geodesy.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

A KNOWLEDGE of the result of the following investigation is so necessary for the correct calculation of the large triangles of modern surveys, and the investigation itself is so easy, that I imagine that an equivalent solution must have appeared already. I have not however seen one, and I therefore think it possible that the publication of my investigation may not be entirely without utility.

The problem which I propose to myself is "To find the curvature-excess upon a surface differently curved in different directions;" the term *curvature-excess* having the same meaning for this surface which *spherical-excess* has for a spherical surface; namely, the quantity by which the sum of the three angles of a triangle (as observed with a theodolite whose axis at each angle of the triangle is made perpendicular to the surface) exceeds 180°. And before entering upon the investigation, I wish to point out that the "geodetic lines" and "geodetic triangles" of speculative geometers have nothing to do with this problem. The triangles with which we are concerned here are the rectilinear triangles formed by the rays of light, proceeding in straight lines (except so far as they are influenced by refraction, which does not sensibly affect their azimuths) from one station to another. The best geometers possessing a practical acquaintance with surveys,—Dalby, Delambre, and Everest,—have fully understood the difference.

I must also point out, that, unless we proceed to an excessive degree of complication, it is impossible to give the solution with the same degree of completeness as for a spherical triangle; because a surface is not defined by a mere knowledge of the principal radii of curvature for one point of the surface, for a higher order of superficial distances from that point than the second. Thus, to take a very simple case, a surface having given principal radii of curvature may be either at the equa-

* *Loc. cit.* vol. lxi. p. 600.

tor of an oblate spheroid, having the greater curvature perpendicular to the equator, or at the equator of a prolate spheroid, having the greater curvature in the equator: but in the former case, on pursuing the line of greatest curvature, we find the curvature gradually diminishing; whereas in the latter case, on pursuing the line of greatest curvature, we find the curvature continuing invariable. It may also be a portion of an infinity of other surfaces, the nature of which, on departing to a sensible distance from the original point, varies in an infinity of different ways.

It is necessary, therefore, to confine ourselves to an approximate solution; and it will suffice to use the approximate formulæ (including with the utmost exactness the terms depending on the second power of the sides of the triangle, but no more) which are applicable to the reduction of observed angles.

Now any surface-triangle, however placed, may be divided into four right-angled surface-triangles (of which one is negative), the sides embracing the right angle being in the direction of the greatest and least curvatures respectively. It will be sufficient to investigate the curvature-excess for one of these right-angled triangles. For it will be found, as a result of the investigation, that the curvature-excess which we are seeking will be expressed by a simple multiple of the area of the right-angled triangle: and as the curvature-excess of the large triangle is readily seen to be the algebraical sum of the curvature-excesses of the four right-angled triangles, and as the area of the large triangle is the algebraical sum of the areas of the four right-angled triangles, it follows that the curvature-excess of the large triangle will be the same multiple of the area of the large triangle.

The process which I shall use is, to compute the reduction of each of the horizontal angles to the chord-angle; the sum of these reductions will be the curvature-excess. The well-known formula for reduction is this: if D and D$'$ be the angular depressions of two signals (expressed in parts of the radius), E the horizontal angle included between them; then the reduction (expressed in parts of the radius) is

$$\mathrm{D}.\mathrm{D}'.\operatorname{cosec}\mathrm{E}-\frac{\mathrm{D}^2+\mathrm{D}'^2}{2}\cot\mathrm{E}.$$

Let v, w, x be the three sides of the right-angled triangle, v and w including the right angle; and let V and W be the radii of curvature in the directions of v and w. Let the angles opposite to v, w, x be called A, B, C (C being $=90°$). In

the multiplication of small quantities we may consider $B=90°-A$ and $x^2=v^2+w^2$.

The linear depression of B below the horizon of C is $\frac{v^2}{2V}$, and therefore its angular depression is $\frac{v}{2V}$. That of C below the horizon of B is sensibly the same.

The linear depression of A below the horizon of C is $\frac{w^2}{2W}$, and therefore its angular depression is $\frac{w}{2W}$. That of C below the horizon of A is sensibly the same.

The linear depression of B below the horizon of A is

$$\frac{v^2}{2V}+\frac{w^2}{2W},$$

and therefore its angular depression is

$$\frac{1}{x}\left(\frac{v^2}{2V}+\frac{w^2}{2W}\right)=x\left(\frac{\sin^2 A}{2V}+\frac{\cos^2 A}{2W}\right).$$

That of A below the horizon of B is the same.

Hence we obtain the following expressions for the three reductions.

For the right angle C, we must make

$$E=90°,\quad \operatorname{cosec} E=1,\quad \operatorname{cotan} E=0,\quad D=\frac{v}{2V},\quad D'=\frac{w}{2W}.$$

The expression for the reduction becomes

$$\frac{vw}{4VW}=\frac{x^2.\sin A.\cos A}{4VW}.$$

For the angle A,

$$D=\frac{w}{2W}=\frac{x.\cos A}{2W},\quad D'=x\left(\frac{\sin^2 A}{2V}+\frac{\cos^2 A}{2W}\right),\quad E=A.$$

Substituting these quantities in the expression for the reduction, it becomes

$$x^2\left\{\frac{\sin A.\cos^3 A}{8W^2}+\frac{\sin^3 A.\cos A}{4VW}-\frac{\sin^3 A.\cos A}{8V^2}\right\}.$$

For the angle B,

$$D=\frac{x.\sin A}{2V},\quad D'=x\left(\frac{\sin^2 A}{2V}+\frac{\cos^2 A}{2W}\right),\quad E=90°-A;$$

and the expression for reduction becomes

$$x^2\left\{\frac{\cos A.\sin^3 A}{8V^2}+\frac{\cos^3 A.\sin A}{4VW}-\frac{\cos^3 A.\sin A}{8W^2}\right\}.$$

The sum of the three reductions, or the curvature-excess, is

$$\frac{x^2. \sin A . \cos A}{2VW}, \text{ or } \frac{vw}{2VW}.$$

The area of the triangle $= \frac{vw}{2}$. Hence the expression for the curvature-excess is

$$\frac{\text{Area of triangle}}{VW}.$$

If we had a triangle of the same area upon a sphere whose radius is R, the spherical-excess would be $\frac{\text{Area}}{R^2}$. Hence the curvature-excess is the same as the spherical-excess of a triangle of the same area on the surface of a sphere whose radius $R = \sqrt{VW}$.

The application of this theorem to the triangles of a terrestrial survey is very easy.

One of the deductions from the general expression is, that the curvature-excess upon a developable surface is $=0$. For there one of the radii of curvature is infinite. This result is very easily verified in the particular case of the cylinder, whatever be the magnitude of the triangle. For if, in the first place, we consider the "geodetic" line upon a cylinder (which, when inclined to the axis, is a regular helix), it will be obvious that its directions at its two extremities make equal angles with the axis of the cylinder; and next, if we consider the positions of the planes of reciprocal vision at the two stations (which are different), and their intersections with the cylindrical surface at the respective points of vision (neither of which intersections coincides with the helix), it will be obvious that the directions of these intersections also make equal angles with the axis of the cylinder; and therefore they are equally inclined (one on one side, and the other on the other side) to the helix; and therefore the sum of the visual azimuthal angles in the visual triangle is equal to the sum of the angles in the helical triangle. But on developing the surface, the helical triangle becomes a rectilinear triangle, or its curvature-excess $=0$; and therefore the curvature-excess of the visual triangle $=0$.

Royal Observatory, Greenwich,
January 10, 1850.

G. B. AIRY.

XI. *Chemical Examination of Lettsomite* (Velvet Ore).* *By* John Percy, *M.D., F.R.S.*†

At the request of Mr. Brooke and Professor Miller (of Cambridge), I have examined chemically some minerals, of which the composition has not hitherto been satisfactorily ascertained.

The first which I have analysed is the velvet copper ore, Mr. Brooke having supplied me with a specimen containing an unusually large proportion of the mineral, yet weighing only 12·8 grs.

First analysis.—The quantity employed, after separating extraneous matter by the aid of a lens, weighed 2·99 grs.

On adding hydrochloric acid, slight effervescence occurred, due to the presence of some minute particles of blue carbonate of copper: only a trace of matter was left undissolved. The whole was then evaporated to dryness. The residue consisted of brown interlacing acicular crystals. Hydrochloric acid was added, and after some time water, when a pale greenish-blue solution was obtained. This was passed through a very small filter, which, being washed and incinerated, gave only 0·01 grain, its own ash included. The filtrate was treated with sulphuretted hydrogen, which produced a brown-black precipitate. This was washed with water containing hydrochloric acid and sulphuretted hydrogen, and then digested with dilute nitric acid. The solution was boiled with potash. The precipitate (CuO), washed and ignited, weighed 1·44 gr.

The last filtrate was reduced by evaporation and treated with chloride of barium. A white precipitate (BaO, SO^3) was obtained, which, washed and ignited, weighed 1·34 gr.

The excess of baryta was separated by sulphuric acid, and ammonia added, when flocculent white matter, like alumina, was precipitated, which, by the addition of hydrosulphate of ammonia, became greenish-black. It was redissolved in hydrochloric acid, boiled with nitric acid, and again precipitated by ammonia. Washed and ignited it ($Al^2 O^3$ and $Fe^2 O^3$) weighed 0·35.

The filtrate, after separation of the alumina and peroxide

* The name Lettsomite has been assigned to this mineral at the request of Mr. Brooke, as an acknowledgement of the interesting and useful mineralogical assistance he has received from W. G. Lettsom, Esq., whose extensive practical knowledge of minerals, and ample collection of specimens, have enabled him to supply information which Mr. Brooke would have found it difficult otherwise to obtain.

† Communicated by the Author.

of iron, was treated with hydrosulphate of ammonia. No further precipitate occurred (absence of zinc).

Second analysis.—Weight of the mineral, carefully freed from extraneous matter by the aid of a lens, 4·25 grs. It was introduced into a very small tube closed at one end; the other end was then drawn out and connected by a caoutchouc joint with a small U-tube filled with chloride of calcium. This was similarly connected with a large U-tube, also filled with chloride of calcium, and communicating with an exhausting pump; so that, by alternately exhausting and allowing the air to enter, the water expelled from the mineral by heat might be collected in the small U-tube and weighed. The tube containing the mineral was heated in an oil-bath at 500° F. for a considerable time. A sensible quantity of water was evolved, and the mineral acquired a dull green colour. The small U-tube increased in weight 0·46 gr., whilst the tube containing the mineral lost 0·45. The difference, 0·01, was probably owing to the air having occasionally been allowed to pass too rapidly from without through the large U-tube to be completely desiccated. On gently heating the tube containing the mineral over a spirit-lamp, a further quantity of water was separated. The heat was increased to dull redness for a few seconds, and the water, which condensed in the upper part of the tube, expelled. This further loss by heat was 0·53. The total loss, therefore, by heat was 0·98, which may be estimated as water, for the water last evolved did not redden litmus. The error, certainly, does not exceed 1·27 per cent., as proved by the following determination of sulphuric acid. The mineral, deprived of water by heat, was digested in dilute nitric acid. Some reddish-brown matter remained undissolved, which, after ignition, and including the ash of a very small filter, weighed 0·10. Chloride of barium was added to the solution. The sulphate of baryta, washed and ignited, weighed 1·74.

The excess of baryta was separated by sulphuric acid. The copper was precipitated by sulphuretted hydrogen, and determined as in the first analysis. The oxide of copper weighed 1·98. It was redissolved in dilute nitric acid, and treated with excess of carbonate of ammonia; but no precipitate occurred after standing many hours.

The alumina was separated from the peroxide of iron by potash. After adding hydrochloric acid to the potash solution of alumina, it was boiled with a little chlorate of potash (Fresenius). Ammonia in slight excess was added. The alumina, washed and ignited, weighed 0·47.

The peroxide of iron was redissolved in hydrochloric acid and precipitated by ammonia; washed and ignited, it weighed 0·05.

In every instance the washing of the precipitates was continued until the wash-water either ceased to be rendered in the slightest degree turbid by nitrate of silver, or to leave an appreciable residue by evaporation.

Phosphoric acid was sought for in the alumina obtained in the second analysis, and detected both by Berzelius's process (fusing with silica and carbonate of soda, and subsequent precipitation by ammonia and chloride of magnesium), and Svanberg's test, molybdate of ammonia. But by the latter test phosphoric acid was also detected in the potash used in the preceding analysis, although prepared with alcohol. And the same test also indicated its presence in the carbonate of potash from which the potash was derived. On testing the mineral directly with molybdate of ammonia, I did not detect phosphoric acid; and I have obtained the same result with another specimen of velvet ore, which I have recently received from Berlin. I am particular in stating these facts, because it is obviously of great importance to the analytical chemist that his potash should be free from phosphoric acid, especially in the separation of alumina from iron*.

Matrix.—It consists chiefly of hydrated peroxide of iron, but contains also alumina, lime, oxide of copper, sulphuric acid, silica, and a trace of phosphoric acid (by Svanberg's test). Here and there upon the surface is some amorphous white matter, which is probably a basic sulphate of alumina, as it contains alumina and sulphuric acid.

Results tabulated.

	First analysis. grs.	Second analysis. grs.
1. Weight of mineral .	2·99	4·25
2. CuO	1·44	1·98
3. $Ba\,SO^3$	$1·34 = 0·46\,SO^3$	$1·74 = 0·60\,SO^3$
4. $Al^2\,O^3$ and }	0·35	0·47
5. $Fe^2\,O^3$ }		0·05
6. HO	...	0·98
7. Matter after heating insoluble dilute NO^5	...	0·10

* I have found, I believe, an *exact* method of separating alumina from iron without the use of potash, which I hope shortly to publish with the requisite analytical proofs.

	I.	Oxygen.	II.		Oxygen.
CuO	48·16	9·71	46·59		9·40
Al^2O^3 Fe^2O^3	11·70	5·17*	11·06 / 1·18	5·14 / 0·36	5·50
SO^3	15·38	9·21	14·12		8·45
HO	23·06	20·49	23·06		20·49
Matters insoluble in dilute NO^5	...	...	2·35		
	98·30		98·36		

Hence the following empirical formula, the iron being omitted as accidental.

$$6CuO,\ Al^2O^3,\ 2SO^3,\ 12HO.$$

Calculated from this formula, the composition is—

$$(CuO=39{\cdot}71,\ Al^2O^3=51{\cdot}44,\ SO^3=40{\cdot}12,\ HO=9).$$

CuO	49·85
Al^2O^3	10·76
SO^3	16·78
HO	22·59
	99·98

The results of the preceding analyses agree very well with each other and with the composition deduced from this formula, when it is considered that different parts of the specimen were operated on, that the quantities employed were necessarily very small, and that it was impossible to separate perfectly the extraneous matter, consisting of brown (Fe^2O^3) and white particles (probably basic sulphate of alumina).

I have not attempted to deduce any *rational* formula, as probably several might be proposed equally plausible.

On a future occasion I hope to present the results of the examination of another substance which frequently accompanies velvet copper ore.

XII. *On the connexion of the Electricity of Condensation with Lightning and the Aurora.* *By* Reuben Phillips, *Esq.*†

[Continued from vol. xxxv. p. 497.]

70. It was found (48.) that by particular management the boiler was rendered positive by the escape of steam. I have since then discovered a more advantageous method of producing this effect.

71. The weight was removed from the lever of the safety-valve, and the weight of the lever only allowed to act on the

* Estimated as Al^2O^3.

† Communicated by the Author.

valve, which admitted of a pressure being maintained in the boiler equal to about 6 lbs. on the inch; everything else was as before (48.). The boiler was connected with the single-leaf electrometer. When the cock was opened a little, so that the steam might escape from the brass jet with very little force, probably a few ounces on the inch, the boiler became feebly positive. On opening the cock more, the positive electricity of the boiler became stronger until the pressure at the jet was increased to what I estimated to be about 3 lbs. on the inch; afterwards the positive electricity of the boiler became less, and at 6 lbs. on the inch the boiler was strongly negative.

72. The larger collector was placed in the steam at a distance of about 6 inches from the brass jet, and united with the single-leaf electrometer. Many of the lower pressures which readily made the boiler positive did not cause the steam to give any electricity to the electrometer; but when the pressure was increased to 2 or 3 lbs. on the inch, a negative charge was given to the electrometer. The negative state of the steam, like the positive state of the boiler, went on increasing, within certain limits, with the pressure. The boiler was always positive when the steam was negative.

73. When the pressure was increased to a degree a little lower than that at which the boiler changed from positive to negative, the steam communicated a positive charge to the collector, even at a distance of a foot from the end of the brass jet.

74. The Armstrong's condenser with water, being interposed between the boiler and jet, did not much alter the above effects; however, I think I may certainly say that the boiler changed from positive to negative at a lower pressure than it did without the condenser. When the boiler was positive, the sound of the steam was a smooth hiss; but as the pressure was increased it changed to a roar, the boiler becoming at the same time negative. With a very feeble roar the boiler was neutral, which neutrality I have occasionally observed without the condenser. The steam was also found to be negative as before; I did not however try to make the steam and boiler both positive with this arrangement.

75. The gun-barrel and brass tube were placed before the jet as formerly (58.), and connected with the boiler and single-leaf electrometer, the steam being at about 6 lbs. on the inch. When the cock was fully opened, the electrometer was powerfully acted on by positive electricity; and on holding the large collector in the steam and then bringing it to this electrometer, it also communicated a positive charge. In order to produce these effects, it was necessary that plenty of water should be

discharged with the steam, which was effected by giving the condenser time to cool before an experiment was made.

76. It is now seen that a simple alteration of the pressure is sufficient to change the electrical state of the boiler from negative to positive, and the steam from positive to negative; and this takes place not in any adventitious manner, for I could as easily and with equal precision make the boiler positive as negative.

77. A platinum blowpipe jet was united to the apparatus for producing an artificial fountain by condensed air; the jet was fixed rectangularly to the pipe of the fountain, and its aperture was very fine; so that when the water was ejected under a pressure of four or five atmospheres, the stream became divided, at a distance of about 12 to 18 inches, into a stream of fine drops. It was found advisable to keep a very fine piece of platinum wire in the aperture of the blowpipe jet, as it prevented its becoming stuffed up, and by moving the wire I could greatly vary the shape of the discharge. The fountain itself consisted of a brass cylinder about 7 inches long and 3 inches diameter, with the two flat ends soldered on. I think this construction should never be employed; for the varying flexion, though invisible, of the brass ends, the thickness of which cannot be seen, appears gradually to destroy the continuity of the solder, and the instrument becomes liable to burst, as mine has done.

78. The larger collector was held by a tube-holder, and connected with the single-leaf electrometer, and water was discharged from the fountain through the platinum jet so that the drops might fall on the wire-gauze, the distance between the gauze and the jet being from one to two feet; the electrometer became charged positively. The intensity of the charge was very variable, depending on the form of the aperture of the jet: when the water issued as a compact stream, the electricity was strong; but when the platinum wire was so placed that the discharge flew into minute drops almost as soon as it left the jet, there was always very little or no electricity obtained. When the distance between the jet and gauze was diminished to 3 or 4 inches, there was no electricity produced. Distilled and common water were used with about equal effect in these experiments; and I found, too, a brass jet might be substituted for one of platinum. This species of electricity was during my experiments frequently so abundant, that I doubt whether a jet of water, properly discharged, perhaps through an Armstrong's jet, would not be a better source of electricity than a jet of steam.

79. The fountain being insulated, the discharge of the water

was found to leave it negative; and when the screen which received the water was also connected with the electrometer, no electrical effects were produced: this, however, at present rests on only one experiment, in which the insulation was not so good as I should have liked.

80. The above method of electrical excitation has an imposing appearance of novelty; but since the ordinary frictional electricity of steam is produced by water rubbing against the discharging passage (Faraday), the above experiments must be regarded as a singular case in which the quantity of water is infinite with regard to the quantity of steam.

81. Steam was discharged from the brass jet as before (71, 72.), and the larger wire-gauze collector was held by means of a tube-holder at distances varying from 6 to 12 inches from the end of the brass jet. The pressure in the boiler was low, and the steam was so slowly discharged from the jet that it imparted no electricity to the screen. A stream of water was now discharged from the platinum jet of the fountain through the current of steam, generally in the same direction as the steam was passing, so that the water might fall on the screen which was connected with the single-leaf electrometer; a negative charge was obtained. As the pressure which drove the steam out was increased, so this negative charge increased in intensity until the steam was about to become positive. In performing these experiments, care was taken to adjust the platinum jet so that the electricity of the discharged water might be little or nothing; otherwise a positive charge was obtained from the water. The length of the column of water and its solidity were greatly varied; and it was thus found advantageous, in order to have a strong negative effect, that the stream of water should be rather compact as it issued from the jet, and that the water should pass over a distance of 2 or 3 feet before it entered the steam. The amount of electricity collected was very variable under apparently similar circumstances, but it was always negative. The state of the boiler was frequently examined, and was always found to be positive. I also discharged the steam through some other jets instead of the above-mentioned brass one, but the results did not vary.

82. This negative charge given to the screen was, I think, only produced by the drops of water collecting negative electricity from the steam, much in the same way as the wire-gauze did (72.).

83. The steam-cloud, instead of being allowed freely to escape, was received into a tin pipe. This pipe was 2·5 inches diameter, and one end formed a right angle to the rest of the pipe; the greatest length of one limb of this pipe was

13 inches, and of the other 3·5 inches. The shorter limb stood before the brass jet, with its central line coinciding with the central line of the path of the steam; the hole of this smaller limb was also fitted with a bung having a hole in its middle 1·3 inch diameter. The longer limb formed an angle of about 25° with the horizon, and was attached to a glass rod by which the tin pipe was insulated. The distance of the end of the brass jet from the nearest part of the bung was 2·5 inches, and the tin pipe was connected with the single-leaf electrometer.

84. The above arrangement having been completed, the fountain was held at about 8 inches from the open end of the longer limb, and a stream of water from the platinum jet directed axially through the long arm of the pipe; no electricity was produced. Then while the water was flowing, the cock of the boiler was turned a little, so that the steam might issue from the jet under some very low pressure, perhaps a foot of water: an abundance of positive electricity was now obtained; but on again shutting off the steam, no electricity. The boiler was uninsulated.

85. I found this electrical effect to increase as the pressure was increased, always taking care to keep within the point at which the boiler would have become negative. But that no doubt should remain as to the state of the boiler, I have often, immediately after having obtained the positive charge as above from the steam, connected the boiler with the electrometer and found it always positive.

86. The longer limb of the tin pipe was now brought to an angle of about 55° with the horizon, and instead of the fountain I employed a pipette, which was kept at about 2 feet from the end of the tin pipe; everything else remained as before. While the steam was escaping into the pipe, as in former experiments (84, 85.), a slender stream of water was blown from the pipette which fell in drops through the steam in the pipe; a positive charge was at once given by the water to the tin pipe, although before the water fell through the steam there was no effect produced on the electrometer. The boiler was positive.

87. An electrical effect was produced by blowing water from the pipette on an insulated screen, the screen becoming positive. The amount of electricity so obtained was very variable under apparently similar circumstances, and frequently nearly nothing for a great number of trials; it was at such times that the above experiments were made, the pipette being examined as to its electrical powers both before and after each experiment.

88. I have not obtained any effect by discharging air with the steam from the boiler. My experiments were usually made by pumping air into the boiler, and discharging the mixture of steam and air through the brass jet, the pressures being generally low. At a pressure of 40 lbs. on the inch, the difficulty of getting air into the boiler was so great that my experiments are not satisfactory; and certain capricious results were obtained, which I feel sufficiently convinced at present were produced by water being deposited in the discharging passages.

89. The arrangement (54.) was restored, and the tube was brought up close to the brass connecting piece, by which the end of the tube was partly closed. The steam was also at 40 lbs. on the inch. The wire-gauze being held in the steam as it issued from the tube, and then removed to the single-leaf electrometer, was found to have become negative. The piece of a gun-barrel was now substituted for the glass tube; the steam was also negative. These two experiments are merely variations of that formerly described (47.). It was singular to observe how suddenly and completely the electrical properties of the steam were changed by moving the gun-barrel or glass tube out from the brass connecting piece.

90. From the foregoing experiments, I think it follows that the electricity of condensation (51, 52, 56, 84, &c.) is developed by drops of water moving either with, or contrary to the electro-current of condensation (11, 14, &c.).

91. I conclude from (90.) that rain is necessary to the formation of lightning, and from (86.) that the drops of rain need not possess any very great velocity.

92. As to the connexion of lightning with rain, I have but to refer to Mr. Birt's paper (Phil. Mag., vol. xxxv. p. 161); and with regard to the connexion of mist with the aurora, I refer to M. De la Rive's paper (Phil. Mag., vol. xxxiv. p. 291).

93. It has been suggested by Sir John Herschel that the light of the sun may be produced by electric currents; and is it impossible that these currents may be of the same nature as our lightning and aurora? The sun has a dark body, an atmosphere, and a difference of temperature.

7 Prospect Place, Ball's Pond Road,
January 10, 1850.

XIII. *On two New Salts of Chromic Acid.* By ARCHIBALD DUNCAN, *Jun., Esq.**

IN 1827, Dr. Thomson described in his paper on the Compounds of Chromium (Phil. Trans. 1827, p. 223), the double salt—*potash chromate of magnesia*—($KO\,CrO_3$, $MgO\,CrO_3$, $2HO$) obtained by digesting a solution of bichromate of potash over carbonate of magnesia. I obtained a corresponding lime-salt about two years ago by the following process. A boiling solution of bichromate of potash was poured over newly-slaked lime in a tall vessel. The undissolved lime having subsided, the supernatant fluid, which was of a lemon-yellow colour, was drawn off by a syphon, and slowly evaporated in a hot-air stove at 80°. During the first two days of the evaporation, crystalline crusts of an *orange* salt were formed on the surface of the liquor, and required to be frequently removed. After this time, however, these crusts ceased to be produced, and crystals of a *yellow* salt began to make their appearance at the bottom of the evaporating bason, and in two or three days more a mass of beautiful crystals was obtained. The proportion of the orange to the yellow salt depends a good deal on the temperature employed in the evaporation. In one experiment the heat was raised to boiling, and no yellow crystals were obtained at all, orange crusts continuing to separate as fast as they could be removed.

Yellow Potash Chromate of Lime.—This salt crystallizes in lemon-yellow four-sided oblique prisms. It is soluble in water, but insoluble in cold alcohol, and is formed in the latter part of the process described.

The salt, when ignited, fuses; and on cooling, the mass has a crystalline aspect, and is quite soluble in water.

The mean of several analyses gave the following result:—

	Experiment.	Calculation.
Chromic acid . . .	51·840	52·52
Potash	23·900	24·24
Lime	14·950	14·14
Water	9·600	9·10
	100·290	100·00

This corresponds nearly with the formula $KO\,CrO_3$, $CaO\,CrO_3+2HO$, the water being slightly in excess. It therefore is a parallel compound to the magnesian salt described by Dr. Thomson.

Orange Potash Chromate of Lime.—The mode of formation

* Communicated by the Author, having been read before the Philosophical Society of Glasgow, Dec. 13, 1848.

of this salt has been already described. It is soluble in water. The mean of three analyses yielded the following result:—

Chromic acid . . .	52·070
Lime	23·990
Potash	17·550
Water	6·230
	99·840

This approaches nearly to the formula $3KO\ 7CaO\ CrO_3\ 5HO$. When this salt is ignited it does not fuse, and when cool its colour is yellow. It does not again altogether dissolve in water, and thence it appears to have undergone decomposition.

XIV. *Remarks on the Weather during the Quarter ending December* 31, 1849. *By* James Glaisher, *Esq., F.R.S., F.R.A.S., and of the Royal Observatory, Greenwich**.

IN my paper upon the meteorological particulars of the quarter ending September 30, 1849, and published in the Philosophical Magazine, I spoke of the great mortality which had existed throughout that quarter in London and its environs. This excess of mortality decreased rapidly; and not less remarkable has been the decrease in the weekly and monthly rate of mortality in the past quarter; the mortality having been, in three successive weeks in November, 269, 284, and 270 below the estimated number. The mortality in October was 557, in November was 1160, and in December was 564 less than the calculated numbers. This decrease is extraordinary; and it proves that the epidemic of cholera carried off many of the sickly and weakly, whose deaths would otherwise have made up the average numbers in the past quarter.

The meteorological returns for the past quarter have been furnished to the Registrar-General from the usual places, and which have passed my usual examination and reduction.

During the past quarter there has been an unusual prevalence of fog, particularly in the month of November. Snow has fallen more frequently than usual, but in small quantities only. The daily temperatures of the air till October 16 and after November 20, with the under-mentioned exceptions, were below their average values; in the former period the mean defect was 4°·2, and in the latter it was 4°·0. Between October 17 and November 14 the temperature was above the

* Communicated by the Author.

average for the season; its mean daily excess was 6°·3. From December 5 to 8 the mean daily excess of temperature was 4°, and between December 14 and 19 the average excess was 6°·9. This warm period of six days was remarkable, as occurring between two very cold periods. The temperature in December was variable; on the 15th day it exceeded the average by 13°·4, and on the 28th it was 13° below.

The mean temperature of the air at Greenwich for the three months ending November, constituting the three autumnal months, was 51°·3; and that of the average from the seventy-nine preceding autumns was 49°·3.

For the month of October was 51°·1, exceeding the average of the seventy-nine preceding years by 1°·8, and that of the preceding eight years by 1°·5.

For the month of November was 44°·1, exceeding the average of the seventy-nine preceding years by 1°·7, and less than that of the preceding eight years by 0°·4.

For the month of December was 39°·1, exceeding that of the average of the preceding seventy-nine years by 0°·3, and being less than the average of the preceding eight years by 1°·3.

The mean for the quarter was 44°·8, exceeding the average of seventy-nine years by 1°·3, and being of the same value as that of the preceding eight years.

The mean temperature of evaporation at Greenwich—

For the month of October was 48°·2; for the month of November was 42°·2; and for the month of December was 37°·9. These values are 0°·8, 0°·9, and 1°·3 *below* those of the averages of the same months respectively in the preceding eight years.

The mean temperature of the dew-point at Greenwich—

For the months of October, November and December, were 45°·1, 39°·8 and 35°·1 respectively. These values are 1°·0, 1°·7 and 2°·1 *below* respectively the averages of the same months in the preceding eight years. The mean value for the quarter was 40°·0, and that of the preceding eight years was 41°·6. The difference of these numbers shows that the air has been less humid than usual.

The mean elastic force of vapour at Greenwich for the quarter was 0·267 inch, being *less* than the average from the preceding eight years by 0·017 inch.

The mean weight of water in a cubic foot of air for the quarter was 3·1 grains, being of the same value as that of the average from the eight preceding years.

The mean additional weight of water required to saturate a cubic foot of air was 0·5 grain.. The average from the eight preceding years was 0·4 grain.

The mean degree of humidity in October was 0·815, in November was 0·860, and in December was 0·903. The averages for the eight preceding years were 0·883, 0·901, and 0·896.

The mean reading of the barometer at Greenwich in October was 29·744 inches, in November was 29·743, and in December was 29·795. These readings are 0·086 *greater*, 0·012 *greater*, and 0·036 *less* respectively than the averages of the same months in the preceding eight years.

The reading of the barometer at Greenwich was 29·33 inches on October 1; increased to 29·64 by 2^d at 9^h P.M.; it then decreased and passed below the point 29 inches on the morning of the 4th to 28·93 by 9^h A.M., which was the lowest reading in the month. The reading then increased to 29·67 by noon on the 7th, decreased to 29·13 by next morning, and then increased to 29·91 by 9 A.M. on the 9th; decreased to 29·37 by 3^h P.M. on the 11th; increased to 29·98 by the evening of the 15th, and with slight fluctuations to 30·54 by noon on the 29th; and this was the highest reading during the month; after this it decreased to 29·42 by the end of the month. The range of readings during the month was 1·61 inch.

In November, the reading, with slight variation, decreased to 28·98 by the evening hours of the 4th; it increased to 30·26 by the morning of the 9th; and these readings were the lowest and highest respectively during the month. It decreased to 29·47 by the 14th; increased to 30·19 by the 17th; decreased with slight exception to 29·21 on the 24th; increased to 30·02 by the 27th; and decreased to 29·69 by the last day. The range of readings during the month was 1·28.

In December the reading increased to 30·01 inches on the 1st; it decreased to 29·30 by the evening of the 2nd; increased to 29·44 by the 4th; decreased to 29·17 by the 5th; increased to 29·70 by the 6th; decreased to 29·29 by the 8th; increased to 30·07 by the 10th; decreased with slight exception to 29·33 by the 18th at 9^h P.M.; on the 19th at 9^h P.M. the reading had increased by 0·80 inch; and on the 26th at 10^h A.M. it was 30·48, which was the highest reading during the month; the reading then decreased to 29·29 by noon on the 28th, and increased to 30·22 by the end of the month. The range of readings during the month was 1·31 inch.

At Stone, on October 1, at 9^h A.M., the reading of the barometer was 29·168 inches, and increasing; it was 29·440 at 6^h P.M. on the 2nd, when it began to decrease; on the 4th at 9^h A.M. it was 28·756; and on the 6th at 9^h A.M. it had increased to 29·451, when it was depressed again, and read

28·940 on the 7th at 6^h P.M.: this reading increased to 29·600 on the 9th at 9^h A.M.; it was 29·179 on the 11th at 6^h P.M.; on the 15th at 6^h P.M. it was 29·796; and it was depressed to 29·561 on the 17th at 9^h A.M.; then it increased to 29·861 on the 18th at 6^h P.M.; on the 26th at 9^h A.M. it was 29·481, when it began to increase to 30·028 on the 28th at 9^h A.M., still increasing; on the 29th at 9^h A.M. it was 30·360; then it decreased to 30·250 on the same day at 9^h P.M., still decreasing; it was depressed to 29·180 on the 31st at 9^h P.M. (which makes a fall of 1·070 inch in forty-eight hours), still decreasing.

On November 1 at 9^h A.M. it was 29·114 inches: this reading increased to 29·314 on the 2nd at 9^h A.M., when it decreased again, and was depressed to 28·730 on the 4th at 9^h P.M.; on the 8th at 3^h P.M. it was 29·996, and it increased to 30·030 on the same day at 9^h P.M., then it began to decrease; it was 30·020 on the 9th at 9^h A.M., still decreasing; it was 29·247 on the 14th at 9^h P.M.; then it increased to 29·953 on the 17th at 9^h P.M.; this decreased to 29·696 on the 18th at 9^h P.M.; on the 20th at 9^h A.M. it was 29·802, when it decreased again; on the 23rd at 9^h P.M. it was 29·000; and on the 27th at 3^h P.M. it was 29·825: this reading decreased to 29·468 on the 30th at 9^h A.M.; then it increased again.

On December 1 at 9^h A.M. it was 29·779 inches; on the 2nd at 9^h P.M. it was 29·020; it increased to 29·248 on the 3rd at 9^h P.M.; then it decreased to 28·986 on the 5th at 9^h A.M.; on the 6th at 9^h P.M. it was 29·451: this reading decreased to 29·025 on the 7th at 9^h P.M., then increased again, and was 29·868 on the 10th at 9 P.M.; on the 13th at 9^h P.M. it was 29·488; it increased to 29·520 on the 14th at 9^h A.M.: this reading decreased to 29·464 on the same day at 9^h P.M. (the dry-bulb thermometer was 53°·8; on the same evening a gale of wind blew from the S.W. to W.); it increased again, and was 29·660 on the 15th at 9^h P.M.; then it decreased, and was 29·149 at 9^h P.M. on the 18th; it was 29·952 on the 19th at 9^h P.M., still increasing; it was 29·971 on the 20th at 12^h A.M.; at 1^h P.M. on the same day it was 29·954; then it increased to 30·002 at 8^h P.M. on the same day, still increasing; it was 30·243 at 11^h A.M. on the 22nd day: from that day the observations were discontinued.

The average weight of a cubic foot of air, under the average temperature, humidity and pressure, was 542 grains; agreeing with the average from the eight preceding years.

The rain fallen at Greenwich in October was 2·7 inches, in November was 1·5, and in December was 2·4. The falls for these three months on an average of thirty-four years, are 3·5, 2·7 and 1·5 respectively.

The average daily ranges of the readings of the thermometer in air at the height of four feet above the soil, for October, November and December, were 15°·1, 11°·7, and 9°·1 respectively. The averages for these three months from the preceding eight years are 13°·0, 10°·3 and 9°·0.

The readings of the thermometer on grass in October was at and below 32° on five nights; the lowest was 25°·5; between 32° and 40° on eleven nights, and exceeded 40° on fifteen nights. In November it was at and below 32° on nineteen nights; the lowest reading was 15°, and above 32° on eleven nights. In December the lowest reading was 10°·8; the readings were below 32° on twenty-three nights, and above 32° on seven only.

At Cardington the mean of all the lowest readings of a thermometer on grass in October was 34°·6, in November was 25°·9, and in December was 26°. The lowest readings in these three months were 19°·5, 8°·0 and 8°·0 respectively. At Nottingham the lowest reading on short grass was 16°.

Fog at Dundee on October 1; at Greenwich on October 2; at Sunderland on October 10; at Darlington and Sunderland on October 16; at Hartlepool and Sunderland on the 17th; at Exeter, Tunbridge and Moffatt, on the 22nd; at Berwick on the 24th; at Dundee on the 25th and 27th; at Dundee, Poole, Reading and Sunderland, on the 29th; at Basingstoke, Plymouth, Greenwich, Oxford and Stone, on the 31st. In November on the 1st at Sunderland; on the 2nd at Greenwich, Birmingham and Edinburgh; on the 3rd all over the country at the same time; on the 4th at Stone; on the 7th at Sunderland; on the 8th at Dundee; on the 11th at London and Stone; on the 12th general over that part of the country south of Stone; on the 13th at Folkestone and Birmingham; on the 17th at Bristol, Southampton, Crewe, Lancaster, Whitehaven and Edinburgh; on the 19th it was general all over the country; on the 20th it was general in the north and at London; on the 21st at Sunderland, Whitehaven, York and Southampton; on the 22nd at Portsmouth and Sunderland; on the 23rd at Folkestone, Conway, Sunderland and Hartlepool; on the 24th all over the country, extending to Glasgow; on the 25th at Greenwich, Stone and London; on the 26th general from Plymouth to Glasgow; on the 27th at Greenwich, Oxford, Gloucester, Southampton, Cambridge, Birmingham, Liverpool and Whitehaven; on the 29th at Edinburgh and Dundee; and on the 30th at London, Birmingham, Moffatt, Hartlepool and Berwick. In December on the 1st at many places in the north, extending to Dundee; in the south at Plymouth, Bridgewater, and at Birmingham;

on the 3rd at Southampton and Swindon; on the 4th at Plymouth and Southampton; on the 6th at Exeter, Southampton and Darlington; on the 8th at Berwick; on the 9th at Greenwich; on the 10th general from Plymouth to Glasgow; on the 11th and 12th at Southampton; on the 13th at Southampton and Hartlepool; on the 14th at a few places in the north and in the south; on the 15th at a few places situated in the north; on the 17th and 18th at Dundee; on the 20th at Glasgow and Lanark; on the 21st at Glasgow, Southampton and Oxford; on the 24th at Oxford and Reading; on the 25th at Southampton; on the 26th it was general in the south; and on the 31st at a few places in extremes north and south. Thus fog has been more or less prevalent on fifty-three days in the quarter.

Meteors were seen on October 3, 8, 9, 10, 12, 13, 14, and November 2 at Nottingham; on November 5 at Nottingham, Cardington and Stone; on November 10 at Nottingham and Cardington; on November 11 and 12 at Nottingham and Latimer; on November 15 at Nottingham; on November 16 at Latimer; on November 25 and December 3 and 4 at Nottingham; on December 5, 8 and 14 at Stone; on December 17 at Cardington; on December 19 at Whitehaven, Nottingham and Durham; on December 20 and 23 at Nottingham; and on December 30 at Hartwell Rectory.

The meteor seen at Nottingham on November 2, at $5^h\ 23^m$ P.M., was about $4'$ of arc in diameter; it was visible for half a minute, and was seen by many persons. That seen on November 5, at $6^h\ 20^m$ P.M., was of the size of a star of the first magnitude, and described a path of 50° in length, which was visible during its whole extent for five minutes; at first its motion was straight, and then curved.

At Stone, on November 5, Mr. Fasel saw a brilliant meteor at $6^h\ 8^m$ P.M.; it started from about 3° above Alpha Ursæ Majoris, and moved to a point at about 8° above Beta Bootis.

On December 19 the meteor seen at Whitehaven was large and brilliant.

At Durham, on December 19, a remarkable meteor was seen in the north, which moved slowly from north-north-west to north-east in a horizontal path, accompanied by a tail; it was in sight about twenty seconds. This meteor was also seen at Edinburgh.

On December 30, at Latimer, the Rev. S. King, at about $5^h\ 45^m$ P.M., saw a very brilliant meteor, which, after travelling with great velocity a space between the Pleiades and Alpha Ceti, burst like a rocket into a multitude of bright fragments, which continued visible some seconds.

At Hartwell Rectory, on December 30, at $5^h 45^m$ P.M., the Rev. C. Lowndes saw a very splendid meteor or globe of bright light. When first seen it was situated south-west of Andromeda. Mr. Lowndes' attention was first attracted to it by the light it gave. Its apparent diameter was about $4'$ of arc. It immediately burst, and emitted a knotted streak of red light, the length of which was from $5°$ to $6°$, and the direction of its motion was north by east at an angle of $20°$ from a vertical line to the zenith.

Solar halos were seen at Greenwich on October 5; at Greenwich and at Nottingham on October 12, with a bright mock-sun at both places; on the 20th at Nottingham, with a bright mock-sun; on the 22nd at Greenwich and at Nottingham; on the 31st at Stone and at Nottingham. In November, on the 3rd and 24th at Greenwich; and on the 28th, December 1 and 25 at Nottingham.

Lunar halos were seen at Greenwich on October 29; at Stone and Cardington on October 30; at Greenwich, Cardington, Norwich and Nottingham on October 31; at Nottingham on November 1; at Greenwich on November 5; at Stone on November 29; and a lunar bow was seen at Exeter on November 30; on December 23 a lunar halo was seen at Greenwich and at Nottingham; and on December 25 at Nottingham.

Lunar coronæ were seen at Stone on October 27 and November 25; and at Greenwich on November 29 and 30.

Auroræ were seen at Cambridge and Stonyhurst on October 15; and on October 22 at Greenwich and at Cardington. The magnets were disturbed on both days.

Thunder was heard, but lightning was not seen, at Cardington and at Nottingham on November 15; and at Cardington on December 16.

Lightning was seen, but thunder was not heard, at Hartwell Rectory on October 7; at Nottingham on October 21; at Cardington on November 6, 9 and 20; and at Norwich on December 12.

Thunder and Lightning at Stonyhurst on November 6; and at Liverpool on November 14.

First frost at Darlington on October 6; Hartwell Rectory on October 9; and in London on November 27.

Sleet fell at Stonyhurst on November 6; at Crewe and Stonyhurst on November 7; at Stone on November 26; at Liverpool on November 29; and at Darlington on December 24.

Hail fell at Nottingham on October 12; at Hartwell, Nottingham and Liverpool on October 13; at Manchester on

November 5; at Stonyhurst on November 6; at Greenwich and Stone on November 7; at Nottingham and Saffron Walden on November 15; at Nottingham on November 27 and December 20; and at Greenwich on December 21.

Snow fell at Stonyhurst and Darlington on October 3; at Shap on October 4; at Lancaster, Darlington, Whitby, Manchester and Stonyhurst on November 6; at Lanark, Edinburgh, Stonyhurst, Stone and Saffron Walden on November 7; on the mountains near Whitehaven on November 15; at Yarmouth, Lynn and Manchester on November 27; at Yarmouth, Lynn and Whitby on November 28; at Lynn, Moffatt and Glasgow on November 29. In December, on the 4th at Stone, Cardington, Nottingham and Saffron Walden; on the 5th at Stone, Cardington and Nottingham; on the 12th at Stone; on the 19th at Greenwich; on the 21st at many places; on the 22nd it was falling all over the country; on the 24th at Basingstoke and Exeter; on the 27th at Yarmouth, Greenwich, Stone, Liverpool (the only instance), at Cardington, Saffron Walden and Chiswell Street, London (the first snow this season); on the 28th it was falling all over the country; on the 29th at Greenwich, Stone and Nottingham; and on the 30th at Greenwich.

The following abstract of the weather at Guernsey has been furnished by Dr. Hoskins, F.R.S.:—

October:—The mean temperature agrees with the average for five years. The rain above the average both in quantity and number of days: the dew-point below the average. This month was on the whole windy and rainy, but not cold.

November:—A finer month than the former: less wind and more sunshine: the temperature about the usual average. Dew-point high: much humidity.

December:—From the 1st to the 10th much rain: towards the end slight sleet and snow showers: wind variable in force. Although the temperature was never below 33°, partial frosts occurred in exposed situations. Nevertheless orange trees, geraniums, and other exotics out of doors, were only slightly injured. Out-of-door grapes are still to be seen in paper bags on the walls of houses. On the whole, the weather, though keen, was not cold. The snow did not lie more than a few hours. Rain fell with greater continuance than usual; as much as 1·505 inch was measured in twenty-four hours. Generally rain occurs in heavy showers, the interval being fine with sunshine, and the surface readily dries.

The direction of the wind at Greenwich was north-east till October 16, passing at the rate of 107 miles daily; it was south-west from October 17 to November 18, with an average

daily motion of 110 miles; it was north-east from November 18 to November 30; its motion was about 60 miles daily; it was then north-east and south-east till December 14, with a daily motion of 100 miles; and it was after this south-west and north-west till the end of the year, passing on the average at the rate of 130 miles daily.

The daily horizontal movement of the air in October and November was 95 miles, and in December it was 110 miles.

During the whole of last quarter an observation at 9^h A.M. Greenwich time has been taken daily at many of the railway stations by the station-masters, and forwarded to London free of expense by the several railway companies, and published on the following day in the 'Daily News,' the proprietors of which paper have incurred the expense of collecting the several returns from the London railway termini and printing them. The several stations for observation were selected by the Astronomer Royal. I visited every station before observations were made, fixed a compass-card, and remained at the station till I felt certain the observations would be made with accuracy. It is evident that much valuable information may be obtained by these means, with reference to the extent and passage of storms over the country, the extent of sky covered by cloud, and the extent of country over which any particular weather prevails.

With the full set of meteorological instruments possessed by the regular observers, who furnish the observations for the Quarterly Meteorological Reports, many of whom have promised co-operation with the above system, by taking a similar set of observations at the same time, whereby other simultaneous meteorological particulars not included in the daily returns will be supplied, I hope in the course of this year to increase the number of stations by the addition of some in Ireland. At all times, I should hope, that on the course of storms being indicated, gentlemen resident in their apparent course before reaching this country, and after leaving it, will supply the wanting particulars, so as to help to trace it from its source to its termination.

The stations included in the returns to the 'Daily News' extend from Plymouth to the south, to Dundee to the north, to Holyhead to the west, and to Yarmouth to the east.

Daily I lay all the particulars as published on a map, from which I extract the following:—

October 1849.	Direction of the Wind. On the south coast.	On the south-east coast.	On the north-east coast.	On the north-west coast.	In the southern counties.	In the midland counties.	In the northern counties.	General Remarks.
1	calm.	n.	n.e.	w.	variable.	n.e.	n.	Overcast. Rain falling in many places.
2	n.	w.	w.	w.	n.e.	calm.	n. & n.w.	Overcast. Fog. Rain.
3	s. & s.w.	calm.	s.w.	calm.	e.	s.e.	calm.	Rain general. Frost at Lanark.
4	calm.	s.w.	light airs.	light airs.	n.w.	n.w.	n.w.	Rain. Snow at Shap.
5	calm.	w.	w.	w.	w.	light airs.	n.w.	Partially cloudy. Frost at Darlington.
6	n.	n.w.	calm.	light airs.	calm.	calm.	calm.	Clear and partially cloudy. Fog.
7	*Sunday.*							
8	n.	n.	n.	n.	n.	n.	calm.	Strong wind on East Coast. Overcast.
9	variable.	n.w.	w.	n.	calm.	calm.	n.w.	Clear and cloudy.
10	calm.	calm.	calm.	calm.	calm.	calm.	calm.	Fog general.
11	n.	e.	e.	n.	n.e.	n.e.	variable.	Chiefly cloudless.
12	n.e.	n.e.	n.e.	n. & e.	n.e.	n.e.	variable.	Partially cloudy.
13	n.	n.e.	n.	n. & e.	n.e.	n.	n.e.	Overcast. Rain.
14	*Sunday.*							
15	n.e.	n.e.	n.e.	calm.	n.e.	n.	e.	Strong wind on East Coast.
16	n.e.	variable.	light airs.	s.e.	e.	e.	w.	A little rain. Frost at Holyhead.
17	s.	s.	s.	s.	s.	s.e.	s.	Principally calm. Fog to North.
18	s.	s.w.	s.w.	s.	s.	s.w.	s.w.	Hard wind on North-west and North-east Coasts.
19	s.e.	calm.	variable.	s.	variable.	s.	variable.	Light airs general.
20	s.w.	w.	w.	s.	calm.	w.	s.w.	Calm in many places to the South.
21	*Sunday.*							
22	calm.	w.	n.w.	variable.	calm.	s.	s.	Fog prevalent.
23	s.w.	s.w.	w.	w.	s.	w.	w.	Calm at many places. Rain in the North.
24	s.w.	s.w.	s.	s.	s.	s.	s.	Light airs and calm. Rain in the North.
25	s.	s.w.	s.w.	s.	s.w.	s.	s.	Rain general.
26	calm.	s.w.	s.w.	w.	w.	s.w.	s.w.	Light airs and calm. Rain at a few places in the South.
27	calm.	s.w.	s.	s.	s.w.	s.	calm.	Rain general.
28	*Sunday.*							
29	calm.	calm.	calm.	variable.	light airs.	s.	s.	Calm and fog.
30	s.e.	s.	s.	s.	s.	s.	s.	Gentle breeze. Overcast.
31	s.w.	s.w.	w.	variable.	s.w.	s.	variable.	Light airs. Calm. Fog.

November 1849.	Direction of the Wind. On the south coast.	On the south-east coast.	On the north-east coast.	On the north-west coast.	In the southern counties.	In the midland counties.	In the northern counties.	General Remarks.
1	s.e.	s.e.	s.e. & e.	s.	s.e.	s.e.	s. & e.	Rain scattered over the country. Fog at Sunderland.
2	s. & s.e.	s.e.	s.	variable.	s.e.	s.	variable.	Gentle breeze and calm.
3	variable.	e.	calm.	s.e.	s.e.	calm.	calm.	Fog general, including Scotland.
5	w.	s.w.	s.w.	w.	s.w.	s.w.	w. & s.w.	Calm and rain in the S. Gale with sleet and snow in the N.
6	variable.	w. & s.w.	variable.	variable.	light airs.	light airs.	light airs.	Calm at many places.
7	s.w.	s.w.	s.w.	variable.	calm.	s.	s.e.	Snow in the North. Rain in the South.
8	s.w.	s.w.	s.w.	w.	s.w.	s.w.	w.	Rain to the North. Fog at many places.
9	s.	s.w.	s.w.	s.w.	s.w.	s.w.	s.w.	Calm to the S. Strong breeze in the Midland Counties. Gentle breeze to the North.
10	s.w.	w.	s.w.	s.	s.	s.	s. & w.	Calm generally.
12	calm.	w.	w.	s.	s.	s.	s.w.	Calm and gentle breezes. Frost at Brighton.
13	s.w.	s.w.	s.	s.e.	s.s.w.	s.s.w.	s. & w.	Rain at many places.
14	s.w.	s.w.	s.w.	w.	s.s.w.	s.s.w.	variable.	Light airs. Rain at many places.
15	n.w.	w.	n.w.	n.w.	w.	n.w.	calm.	Gentle breeze to the S. Gale in the Midland. Calm to the N.
16	n.w.	n.	n.w.	n.w.	n.w.	n.w.	variable.	Calm and light breeze. On the coast, strong breeze.
17	n.	n.n.w.	variable.	variable.	light airs.	calm.	variable.	Calm at many places. Fog prevalent.
19	n.	n.	n.w.	calm.	calm.	calm.	calm.	Fog prevalent.
20	calm.	calm.	calm.	calm.	calm.	calm.	calm.	Fog general. Rain to the North.
21	s.e.	e.	s.	variable.	calm.	calm.	calm.	Fog and rain.
22	s.e.	s.e.	s.	s.	s.e.	s.e.	s.	Calm generally. Rain and fog at a few places.
23	s.w.	s.w.	s.	s.w.	calm.	s.	s.	Light airs and calm. Rain everywhere.
24	calm.	calm.	calm.	calm.	calm.	calm.	calm.	Fog prevalent.
26	calm.	e.	s.e.	s.e.	calm.	calm.	calm.	Fog at many places. Gale at Yarmouth.
27	light airs.	s.w.	variable.	variable.	variable.	calm.	calm.	Fog and frost general.
28	variable.	variable.	variable.	variable.	variable.	variable.	variable.	Fog and sharp frost everywhere.
29	s.e.	s.e.	variable.	s.	s.e.	s.e.	calm.	Light airs. Snow to the North.
30	s.w.	s.w.	w.	s.	calm.	s.	s. & s.w.	Calm with rain to the South. Calm and fog to the North.

December 1849.	Direction of the Wind.							General Remarks.
	On the south coast.	On the south-east coast.	On the north-east coast.	On the north-west coast.	In the southern counties.	In the midland counties.	In the northern counties.	
1	variable.	n.w.	variable.	s.w.	calm.	s.	variable.	Light airs and calm. Fog in many places.
3	calm.	e.	e.	s.e.	calm.	e.	e.	Calm in South, a gale in North : see remarks.
4	n.	n.w.	n.	n.e.	calm.	n.	n.e.	Calm generally. Fog. Frost. Snow.
5	s.	s.e.	s.w.	s.e.	s.e.	s.e.	variable.	Calm. Rain. Snow. Frost.
6	s.	s.	variable.	s.e.	s.e.	s.e.	s.	Variable. No frost.
7	s.e.	s.e.	s.e.	s.e.	s.e.	s.e.	e.	Strong breeze on the coast. Heavy rain at many places.
8	w.	w.	e.	variable.	w.	s.w.	e.	Rain general.
10	calm.	calm.	calm.	calm.	calm.	calm.	calm.	Fog all over the country.
11	n.	e.	n.e.	s.e.	variable.	variable.	variable.	Light fog. Slight rain.
12	n.e.	e.	e.	e.	s.e.	e.	e.	Generally overcast.
13	s.e.	s.e.	variable.	s.e.	s.e.	s.e.	s.e.	Slight rain. Frost. Hail.
14	s.w.	s.w.	s.w.	s.	variable.	s.w.	s.w.	Rain to the South. Light airs to the North.
15	s.w.	s.w.	w.	s.w.	s.w.	s.	w.	Rain to the South. Calm and fog to the North.
17	w.	w.	w.	w.	w.	w.	w.	Strong breeze to the South. Gentle airs to the North.
18	s.w.	s.w.	s.e.	s.	s.	s.	variable.	Rain falling at many places.
19	n.w.	n.w.	n.w.	n.w.	n.w.	n.w.	n.w.	Strong wind generally.
20	n.w.	n.w.	n.w.	n.w.	n.w.	n.w.	n.w.	Sharp frost and snow.
21	n.e.	n.e.	n.e.	n.e.	calm.	variable.	n.e.	Snow on the East Coast. Fog near the S. coast. Hard wind in [Midland Counties.
22	n.e.	n.e.	n.e.	e.	n.e.	n.e.	variable.	Snow everywhere.
24	n.w.	n.w.	n.e.	n.w.	calm.	n.w.	variable.	Calm general. Snow and sleet.
25	n.e.	n.e.	variable.	variable.	n.e.	calm.	n.w.	Hard frost at Shap.
26	n.w.	w.	n.w.	n.	n.w.	n.w.	variable.	Calm general. Fog prevalent.
27	n.w.	n.w.	n.w.	n.w.	n.w.	n.w.	n.w.	Frost and snow at some places.
28	n.	n.w.	n.w.	n.	n.w.	n.w.	n.w.	Hard frost and snow everywhere.
29	n.w.	n.w.	n.w.	n.w.	n.w.	n.w.	n.w.	Hard frost and snow.
31	n.	n.w.	n.w.	variable.	calm.	calm.	variable.	Fog prevalent and frost.

On November 6 a stream of air was passing from the Irish Sea, described as a strong breeze at Holyhead, Liverpool and Manchester; as a storm at Lancaster, a heavy gale at Whitehaven, and a gale at Durham: the breadth of this stream was about 2°. It was not felt on the eastern side of the Cumberland mountains.

On November 15, in latitude $53\frac{1}{2}°$ on the west coast, a hard wind blew from the Irish Sea, described as a hard wind at Holyhead, a gale at Crewe, and merely as a strong breeze at Birmingham, and as a gentle breeze only before it reached Northampton or Oxford, towards which places the air was travelling. At the same time it was described as a calm at Liverpool.

On December 3 a hard wind was blowing over that part of the country extending from Dundee to Manchester, and which was described as a storm at Hartlepool, as a heavy gale at Sunderland and at Yarmouth, and as a gale at Crewe. At all places situated south of Birmingham the air was described to have been either in very gentle motion, or calm, with rain falling at some places, and fog prevalent at others. Tracing the course of this storm, it seems to have reached England at Yarmouth, and from thence passed in a south-east direction up the country, it being most severe on the eastern coast, and so on to Dundee; this storm was probably felt in Belgium; and observations at Brussels, and at other places in its apparent course, would be valuable.

On December 8 a strong wind was blowing from the Irish Sea, affecting Holyhead, Liverpool, and described as a storm at Lancaster; in the line continued which joins Holyhead and Lancaster, viz. at Darlington and at Hartlepool, the wind was blowing strongly; its direction however at these places was from the east, or towards the Irish Sea. The whole mass of air north of this storm was moving from east to west, and at places south of the storm it was moving from west to east. On this day Irish observations are necessary to follow this storm.

In this way data may be collected for very important additions to our knowledge of meteorological phænomena.

I have been favoured with the following reports upon agriculture.

From Stonyhurst, by the Rev. A. Weld.

The potatoe disease made no further progress after about the middle of October; even those that were tainted in the getting-up have remained nearly in the same state, except that the disease seems to have taken rather the form of a dry rot, without however extending any further.

The crop of Swedes was very abundant, averaging upwards

of fifty tons per acre. Mangel-wurzel was also a very good crop.

In general the season has been favourable for the usual winter occupations, as draining, felling timber, hedging, leading manure, &c. The late severe weather has for the most part interrupted out-door work.

From South Hampshire, by John Clark, Esq. near Romsey.

The seed-time for turnips was dry. The crop was in many instances light and uneven. The subsequent fine rains and weather have caused all kinds of root and grass to grow vigorously. There has been an abundance of food for cattle, which have thriven well.

The seed-time for wheat was all that could be desired. A large breadth of land is sown, and the plant is looking well. There has been a good deal of employment for labourers, and consequently there has been a far less number unemployed than in many seasons past.

The monthly mean values of the several subjects of investigation are published in the Registrar-General's Quarterly Report, and subjoined are the mean values for the quarter. The observations have been corrected for diurnal ranges, and the hygrometrical results have been deduced from my tables, and the results are all comparable with each other.

The mean of the numbers in the first column is 29·680 inches, and this value may be considered as the pressure of dry air for England during the quarter ending December 31, 1849.

The mean of the numbers in the second column for Guernsey and those places situated in the counties of Cornwall and Devonshire, is 48°·3; at Liverpool and Whitehaven is 44°·2; for those places situated south of latitude of 52°, including Chichester and Hartwell, is 44°·3; for those places situated between the latitudes of 52° and 53°, including Saffron Walden and Holkham, is 43°·2; for those places situated between the latitudes of 53° and 54°, including Derby and Stonyhurst, 42°·1; and for Durham and Newcastle is 42°·2. These values may be considered as those of the mean temperatures of the air for those parallels of latitude during the quarter ending December 31, 1849.

The average daily range of temperature in Cornwall and Devonshire was 9°·5; at Liverpool and Whitehaven was 7°·0, south of latitude 52° was 11°·6; between latitudes 52° and 53° was 11°·4; between 53° and 54° was 11°·6; and north of 54° was 10°·0.

The greatest mean daily ranges of the temperature of the air took place at Nottingham, Aylesbury, Exeter, Beckington and Latimer; and the least occurred at Guernsey, Whitehaven, Liverpool and Torquay.

Meteorological Table for the Quarter ending December 31, 1849.

Names of the places.	Mean pressure of dry air reduced to the level of the sea.	Mean temperature of the air.	Highest reading of the thermometer.	Lowest reading of the thermometer.	Mean daily range of temperature.	Mean monthly range.	Range of temperature in the quarter.	Mean temperature of the dew-point.	Wind.		Mean amount of cloud.	Rain.		Mean weight of vapour in a cubic foot of air.	Mean additional weight of vapour required to saturate a cubic foot of air.	Mean degree of humidity.	Mean whole amount of water in a vertical column of atmosphere.	Mean weight of a cubic foot of air.	Height of cistern of the barometer above the level of the sea.
									Mean estimated strength.	General direction.		Number of days on which it fell.	Amount collected.						
	in.	°	°	°	°	°	°	°					in.	grs.	grs.		in.	grs	fcet.
Guernsey	29·703	49·2	65·0	33·0	6·1	21·6	32·0	46·1	2·2	s.w.	..	59	16·9	3·8	0·4	0·906	3·6	538	123
Helston	29·637	48·5	69·0	27·0	11·1	28·7	42·0	45·1	1·6	s.w.	6·6	48	13·2	3·7	0·6	0·869	4·4	539	106
Falmouth	....	49·1	68·0	26·0	10·3	28·3	42·0	..	1·4	Variable.	6·8	53	11·3						
Truro	....	48·0	63·0	26·0	8·3	24·3	37·0	..	0·8	s.	7·2	56	15·0						
Torquay	....	48·9	69·0	26·0	8·2	26·7	43·0	42·3	2·1	s.w. & e.	..	45	9·3	3·3	0·9	0·807	3·5	542	120
Exeter	29·738	46·2	68·5	20·0	13·3	36·9	48·5	42·8	1·7	n.	5·4	47	8·6	3·4	0·5	0·874	4·1	542	140
Chichester	....	44·3	65·0	15·0	10·3	34·3	50·0	..	..	s.w. & n.w.	..	..	8·4						
Southampton	29·701	44·9	64·2	11·0	10·3	38·2	53·2	44·9	0·2	......	5·8	40	12·1	3·2	0·5	0·866	3·9	545	55
Beckington	29·804	43·3	67·0	15·0	13·1	40·0	52·0	40·0	..	s.w.; w. & e.	6·4	41	6·3	3·1	0·8	0·766	3·3	543	265
Royal Observatory, Greenwich	29·687	44·8	69·7	18·8	12·0	38·0	50·9	40·0	..	s.w. & n.e.	..	49	6·6	3·1	0·5	0·859	3·7	542	159
Maidenstone Hill, Greenwich	29·710	44·4	64·3	18·8	9·1	34·7	45·5	44·4	..	......	6·8	49	6·7	3·1	0·5	0·866	3·7	544	107
Chiswell Street, London	....	47·3	67·0	22·5	9·0	30·2	44·5	39·2	..	s.w.	..	38	5·7	3·0	1·0	0·744	3·6	540	
St. John's Wood	29·709	44·8	69·0	18·0	13·0	38·3	51·0	41·4	1·7	s.w. & n.e.	8·1	36	4·5	3·3	0·5	0·883	3·7	542	250
Latimer Rectory	29·694	43·6	65·5	17·0	13·1	38·8	48·5	39·6	1·3	n.w.	6·7	44	7·2	3·1	0·5	0·873	3·6	539	335
Aylesbury	29·625	43·5	67·0	18·0	14·0	38·7	49·0	39·3	0·4	s. & n.w.	7·0	41	6·0	3·0	0·5	0·865	3·6	541	280
Stone Observatory	29·643	43·8	66·6	17·0	11·5	38·0	49·6	43·4	1·0	s.w. & n.e.	6·8	43	5·2	3·1	0·4	0·877	3·7	539	320
Hartwell (near Aylesbury)	29·651	43·7	..	17·0	..	..	..	43·7	0·9	......	..	55	7·3	3·2	0·4	0·883	3·8	541	260
Hartwell Rectory	29·651	43·4	65·5	19·0	12·1	41·5	46·5	38·1	0·8	s.w.	4·4	37	..	2·9	0·5	0·846	3·5	541	260
Saffron Walden	....	43·2	65·0	20·0	11·9	36·0	45·0	..	..	s.	..	25	..						
Oxford	29·743	43·9	..	15·3	..	..	..	41·2	2·0	Variable.	7·1	38	4·9	3·2	0·4	0·902	3·8	542	250
Hereford	....	42·2	..	..	..	..	..	..	..	Variable.	..	42	8·6						
Cardington	29·785	42·8	63·6	20·0	12·3	35·3	43·6	40·6	..	Variable.	6·8	47	5·9	3·2	0·4	0·909	3·8	545	200
Norwich	29·638	43·5	63·0	21·0	10·1	32·0	42·0	40·0	..	s.w.	6·9	44	9·2	3·1	0·5	0·873	3·7	544	39
Holkham	29·652	43·4	65·0	18·3	10·9	34·6	46·7	38·4	0·9	s.w.	7·0	62	9·5	2·9	0·6	0·835	3·5	546	31
Derby	....	42·1	64·0	18·0	11·6	35·7	46·0	37·5	..	......	..	56	9·3	2·8	0·7	0·827	3·3	544	
Highfield House, Notts.	29·628	40·8	69·0	17·5	14·4	38·3	51·5	38·3	0·8	s.w. n.w. & n.e.	6·5	60	8·4	2·8	1·0	0·827	3·5	544	103
Manchester	29·666	43·5	..	..	..	..	..	40·0	..	......	..	..	..	3·2	0·3	0·902	3·7	541	110
Liverpool Observatory	29·658	44·5	64·8	22·8	7·5	29·5	42·0	38·7	1·2	s.e.	6·0	..	10·4	3·0	0·5	0·855	3·6	544	37
Wakefield Prison	29·680	42·5	67·5	20·0	12·5	37·2	47·5	37·6	..	s.w.	..	64	8·7	2·8	0·6	0·832	3·4	545	113
Stourton Lodge (near Leeds)	29·701	41·8	64·0	20·0	11·7	34·0	44·0	39·5	1·4	......	7·8	57	10·7	3·0	0·3	0·932	3·6	545	148
Stonyhurst Observatory	29·693	41·8	66·0	20·9	11·9	35·1	45·1	37·5	1·1	s.w. & n.e.	6·7	48	13·9	2·8	0·4	0·852	3·4	540	381
Whitehaven	29·574	43·9	64·0	25·0	6·6	28·3	39·0	40·7	3·1	s.w.	..	56	12·7	3·2	0·5	0·892	3·8	542	80
Durham	29·655	41·6	62·1	19·8	9·2	31·3	42·3	38·3	1·3	......	5·9	50	..	2·9	0·4	0·853	3·5	540	347
Newcastle	29·648	42·7	65·5	23·0	10·7	30·8	42·5	38·9	..	s.e. & n.w.	..	41	7·9	3·0	0·5	0·834	3·5	544	121
Number of columns	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19

The highest temperature of the air was about 69° at several places; the lowest readings were 11° at Southampton, 15° at Beckington and Chichester, and 15°·3 at Oxford. The extreme range of temperature of the air during the quarter in England was therefore about 58°.

The average quarterly range of the reading of the thermometer in Cornwall and Devonshire was 40°·7; at Liverpool and Whitehaven was 48°·5; south of latitude 52° was 49°·2; and north of this parallel was 45°·1.

The mean temperature of the dew-point in Cornwall and Devonshire was 44°·1; south of latitude 52° was 41°·3; between the latitudes 52° and 53° was 40°·1; and north of 53° was 38°·7.

The amount of cloud was such as to cover about three-fourths of the sky nearly.

Rain has fallen on the greatest number of days at Wakefield, Holkham, Nottingham and Guernsey; the average number at these places was 61. It fell on the least number of days at Saffron Walden, St. John's Wood, Hartwell Rectory and Oxford; and the average number at these places was 34. The stations at which the largest falls have taken place are Guernsey, Stonyhurst, Helston and Southampton. The smallest falls occurred at St. John's Wood, Oxford, Stone and London. The average fall in Guernsey, Cornwall and Devonshire was 12·4 inches; south of latitude 52° was 6·9 inches; between latitudes 52° and 53° was 7·6 inches; between 53° and 54° was 10·2 inches; at Liverpool and Whitehaven was 11·6 inches; and at Newcastle was 7·9 inches.

The numbers in the columns 14 to 18 show the mean values of the hygrometrical results; from which we find that—

The mean weight of vapour in a cubic foot of air at all places (excepting Cornwall and Devonshire) in the quarter ending December 31, 1849, was 3·0 grains.

The mean additional weight required to saturate a cubic foot of air was 0·5 grain.

The mean degree of humidity (complete saturation $=1$) was 0·857.

The mean amount of vapour mixed with the air would have produced water, if all had been precipitated at one time on the surface of the earth, to the depth of 3·6 inches.

The mean weight of a cubic foot of air under the mean pressure, temperature and humidity, was 543 grains at the mean height of 182 feet.

And these values for Cornwall and Devonshire were 3·5 grains; 0·6 grain; 0·864; 3·9 inches; and 540 grains, at the average height of 122 feet.

Comparison of the Meteorological Particulars of the Quarters ending December 31, in the Years 1847, 1848 and 1849, in four different Parallels of Latitude.

For the quarter ending Dec. 31.	Mean pressure of dry air reduced to the level of the sea.	Mean temperature of the air.	Mean highest readings of the thermometer: 3 observations.	Mean lowest readings of the thermometer: 3 observations.	Mean daily range of temperature.	Mean monthly range of temperature.	Mean quarterly range of temperature.	Mean temperature of the dew-point.	Wind. Mean estimated strength.	Wind. General direction.	Mean amount of cloud.	Rain. Mean number of days on which it fell.	Rain. Mean amount collected.	Mean weight of a cubic foot of vapour.	Mean additional weight of vapour required to saturate a cubic foot of vapour.	Mean degree of humidity.	Mean whole amount of water in a vertical column of atmosphere.	Mean weight of a cubic foot of air.	Mean height of stations above the level of the sea.
For the Counties of Cornwall and Devonshire.																			
	in.	°	°	°	°	°	°	°					in.	grs.	grs.		in.	grs.	feet.
1847.	29·599	48·8	64·5	28·7	8·6	....	35·7	....	1·3	s.w.	7·3	54	16·1	3·8	0·3	0·914	4·8	537	122
1848.	29·676	48·5	67·6	30·0	7·5	....	37·5	43·3	1·3	s.w.	6·6	56	12·9	3·5	0·6	0·839	4·1	540	122
1849.	29·693	48·3	67·1	26·3	9·5	27·7	40·7	44·1	1·6	s.w.	6·5	50	12·4	3·5	0·6	0·864	3·9	540	122
South of latitude 52°, exclusive of Cornwall and Devonshire.																			
1847.	29·658	46·5	68·9	24·2	11·3	....	45·1	....	1·5	s.w.	7·1	44	7·4	3·5	0·3	0·909	4·3	539	172
1848.	29·662	44·7	71·8	23·0	12·1	....	48·0	41·5	1·1	s.w.	6·7	50	8·6	3·3	0·4	0·892	3·8	540	205
1849.	29·691	44·3	66·4	17·3	11·6	37·3	49·2	41·3	0·9	s.w.	6·5	44	7·0	3·1	0·6	0·848	3·6	552	229

Between the latitudes of 52° and 53°.																		
1847.	29·696	45·2	68·3	32·8	10·7	....	41·0	....	1·4	s.w.	6·8	38	7·5	3·3	0·4	0·888	4·1	539	109
1848.	29·617	44·7	72·3	23·2	10·0	....	49·2	41·8	....	s.w.	6·1	50	8·4	3·3	0·4	0·883	4·0	543	88
1849.	29·704	43·2	64·2	18·9	11·3	34·5	44·3	40·1	1·4	s.w.	6·9	43	7·6	3·1	0·5	0·879	3·7	549	130
Between the latitudes of 53° and 54°.																		
1847.	29·608	45·9	61·8	27·5	9·9	....	37·3	....	1·4	s.w.	7·4	47	8·1	3·3	0·5	0·881	4·0	537	103
1848.	29·610	43·3	69·5	21·0	10·6	....	48·3	38·2	1·1	s.w.	7·3	61	9·0	3·1	0·5	0·868	3·6	541	139
1849.	29·674	42·1	66·1	19·3	12·4	36·1	44·8	38·4	1·1	s.w.	7·0	57	10·2	2·9	0·5	0·878	3·5	543	149
Near the sea coast in the West of England, between the latitudes of 53½° and 54½°.																		
1847.	29·575	47·7	64·6	32·4	6·6	....	32·2	....	1·1	variable.	7·1	47	10·7	3·2	0·7	0·855	3·9	539	37
1848.	29·597	44·9	65·2	28·2	6·9	....	36·7	40·1	1·9	s.w.	7·0	55	10·5	3·2	0·5	0·869	3·7	542	37
1849.	29·616	44·2	64·4	23·9	7·0	28·9	40·5	39·7	2·2	s.w. & s.e.	6·0	56	11·6	3·1	0·5	0·873	3·7	543	58
North of latitude of 54°.																		
1847.	29·605	....	62·2	26·1	9·8	....	36·1	....	1·2	s.s.w.	7·2	35	9·6	3·3	0·2	0·943	4·0	538	281
1848.	29·593	43·0	68·8	23·2	9·9	....	45·6	39·6	1·7	s.s.w.	6·1	47	8·8	3·1	0·4	0·882	3·6	541	230
1849.	29·652	42·2	63·8	21·4	9·9	31·0	42·4	38·6	1·3	....	5·9	46	7·9	2·9	0·4	0·844	3·5	542	234

For similar Tables for the Quarters ending March 31, June 30, and September 30, see the Philosophical Magazine for November 1849.

XV. *On the Method of developing an Incommensurable Fraction given in* Colson's *edition of* Sir Isaac Newton's *Fluxions. By* J. R. Young, *late Professor of Mathematics, Belfast**.

THE paper on Incommensurable Fractions, inserted in the last Number of this Journal, suggests a modification of Colson's rule for expeditiously converting such fractions into circulating decimals, which will, I think, be regarded as a slight improvement.

The fraction chosen by Colson, to illustrate his process, is $\frac{1}{29}$; which he developes, in the usual way, till he arrives at a remainder consisting of but one figure: this remainder, with the 29 underneath, is then appended to the partial development, and the true value of $\frac{1}{29}$, consisting of a certain number of decimals, with a supplementary fraction, is thus exhibited. The whole is then multiplied by the numerator of this fraction, and as many additional decimals are obtained, together with a new supplemental fraction.

The whole row of decimals now furnished, with the new fraction appended, is, as before, multiplied by the numerator of this fraction; and the extent of the row again becomes doubled, and another supplemental fraction presents itself, the numerator of which forms a new multiplier; and so on, till the circulating period is completed.

Now, in certain cases, there is much inconvenience in thus changing the multipliers at every step; for although the multiplier with which we commence may be but a single figure, the subsequent multipliers may, some of them, consist of two figures, or indeed of any number of figures which do not cause the multiplier to be so great as the denominator of the original fraction.

It is an obvious inference from the paper referred to above, that whenever a convenient multiplier is reached, we may, if we please, keep to that multiplier to the end of the process; and by so doing, we shall add on the same invariable number of decimals at each step taken from the stage at which our multiplier was selected, namely the number of decimals furnished at that stage. If, as we proceed, a multiplier still more convenient offers itself, that at first chosen may be replaced by it; and, as before, new rows of decimals will be added at each subsequent step, the number of figures in every addend being the same as the number in the row which sup-

* Communicated by the Author.

plied the multiplier; and it is to be observed that each addend is obtained by merely multiplying *the preceding addend,* and not the whole preceding row.

In the instance chosen by Colson, the several multipliers which he employs, one after another, happen to be conveniently small: it is not so with the fraction $\frac{1}{31}$. Treated in the way here suggested, the development of this fraction is as follows:—

$\frac{1}{31} = \cdot 03\frac{7}{31}$. Here the multiplier is 7, and to this we may keep throughout.

$= \,..\, 22\frac{18}{31}$

$= \quad ..\, 58\frac{2}{31}$. Here the multiplier 7 may be changed for 2.

$= \cdot 032258{,}064516\frac{4}{31}$

$= \,.\,.\,.\,.\,.\,.\,.\,.\,.\,.\,.\, 129032\frac{8}{31}$

$\therefore \frac{1}{31} = \cdot 032258064516129032\frac{8}{31}$

By Colson's method, the multipliers for this fraction are 7, 18 and 14; and although, in that method, the number of decimals is doubled at every step, yet this is no advantage; for in both methods n new figures require n multiplications: the only point of difference between the two methods is this, namely:—in Colson's method we are to employ the successive multipliers as they arise, whereas in the modification of it, here proposed, we may reject those multipliers which are inconveniently large, and use only the smallest.

After I had completed my former paper, a vague recollection revived in my mind of having somewhere seen a process, by Colson, having some analogy to the speculations with which I had been occupied; and I delayed the communication of the paper till I had sought for information on the matter from a distinguished mathematical friend, who was not able, however, to call anything to his remembrance, in connexion with Colson's name, at all allied to what I had been doing. I have since consulted Newton's Fluxions, and have there discovered what I must have read many years ago; and the present supplementary communication is the result of my examination of Colson's rule. This rule admits of an application and an extension of more general utility than the author seems to have conceived; and it is chiefly to show this, that I submit

the present short paper to the readers of the Philosophical Magazine. The extension spoken of is implied in this statement, namely: if we develope *any* fraction by division, whatever be the numerator or denominator, and arrive at a remainder which is either a multiple or a submultiple of a former remainder, we may discontinue the operation and thenceforward proceed by Colson's rule. A single illustration will suffice to confirm this principle.

Let us take the fraction proposed by Metius for the ratio of the circumference of a circle to its diameter, namely $\frac{355}{113}$. In performing the division, the first remainder we get is 16, and the ninth remainder is 4, the quotient up to this remainder being 3·14159292. Consequently

$$\frac{355}{113} = 3{\cdot}14159292\,\frac{4}{113}.$$

But

$$\frac{355}{113} = 3\,\frac{16}{113}$$

$$\therefore\ \frac{16}{113} = {\cdot}14159292\frac{4}{113}$$

$$\therefore\ \frac{4}{113} = 03539823\,\frac{1}{4}\left(\frac{4}{113}\right)$$

$$\therefore\ \frac{1}{4}\left(\frac{4}{113}\right) = 00884955\ \ 75\,\frac{1}{4^2}\left(\frac{4}{113}\right)$$

$$\therefore\ \frac{1}{4^2}\left(\frac{4}{113}\right) = 00221238\ \ 9375\,\frac{1}{4^3}\left(\frac{4}{113}\right);$$

and so on. Consequently

$$\frac{355}{113} = 3{\cdot}14159292035398230088495575221238\ \ 9375\,\frac{1}{4^3}\left(\frac{4}{113}\right)$$

$$\underline{00055\ \&c.}$$

$$\ldots\ 93805\ \&c.$$

where it is to be observed, that the figures cut off are those under which the decimals, arising from the supplemental fraction, are to be placed whenever we choose to put a stop to the process. In the present case these decimals are 00055&c.

The above form has been given to this process for the purpose of preserving an analogy to Colson's method; but, after having got the first group of decimals ·14159292, by common division as above proposed, we may discover the following groups, as fast as they can be written down, by continually

dividing the preceding by 4; and in this way we get at once

$$\frac{355}{113} = 3{\cdot}1415929203539823008849557522123\\ 893805309734513274336283185840\\ 707964601769911504424778761061\\ 946902654867256637168\ 14159292$$

where the decimals recur after the 8 in the 112th place. If the remainder at which we stop, in the operation of determining the leading decimals by common division, be a multiple instead of a submultiple of a preceding remainder, then the continuation of the development would be obtained in a manner analogous to that by which the development of $\frac{1}{31}$ is obtained above. And even should the remainder, b, at which we stop, be neither a multiple nor a submultiple of a preceding remainder, a, yet the development may be carried on upon the same principle; the multiplier, by aid of which each group of decimals is derived from the preceding group, being always $\frac{b}{a}$.

Had the arithmetical facilities noticed in this paper occurred to the older mathematicians, who spent so much time in approximating to the ratio of the circumference of a circle to its diameter, much of that time would have been spared.

London, Jan. 14, 1850.

XVI. *On Percylite, a Mineral not hitherto described.* *By* H. J. Brooke, *Esq., F.R.S.**

I RECEIVED many years since from Mr. Heuland a very small specimen of an undescribed sky-blue mineral in minute cubes, accompanied by gold, on a matrix of quartz and red oxide of iron, which was said to have come from La Sonora in Mexico.

In consequence of the quantity of the mineral on the specimen being too small for analysis without destroying the specimen itself, the mineral has remained until this time unexamined. But it having been pointed out to me by Mr. Lettsom that there was a much larger one in the British Museum, BM
placed among those of gold, from which, without the slightest 890
injury to it, a sufficient portion could be taken for analysis, I applied through Mr. Konig to the Trustees of the British Museum, and obtained their permission to detach the small quantity that has been analysed by Dr. Percy.

* Communicated by the Author.

His analysis is detailed in the following notice; and I am happy in the opportunity thus afforded me, by calling the mineral Percylite, of permanently associating his name with the science of mineralogy, to which his chemical labours have already contributed so much valuable assistance.

H. J. B.

Chemical Examination of the Blue Crystals.
By John Percy, *M.D., F.R.S.*

1. By slight heat the blue colour changes to emerald-green, and reappears on cooling.

2. Heated in the closed tube, the mineral decrepitates, and gives off a little water, which does not redden litmus paper. It readily melts into a brown liquid, which diffuses itself in drops over the contiguous surface of the tube, and on cooling becomes a pale brown fissured mass.

3. Heated in the open tube, white, not sensibly odorous, vapour is evolved.

4. Heated in the outer flame of the blowpipe, the flame is green within and deep blue at the edges.

5. Heated on charcoal in the inner flame, metallic globules are obtained, some having the appearance of lead, others that of copper, or a mixture of copper and lead.

6. Heated with carbonate of soda on charcoal in the inner flame, metallic globules are obtained, which dissolve without residue in dilute nitric acid.

Iodide of potassium throws down from this solution a fine yellow precipitate. Ammonia renders it blue. Hydrochloric acid precipitates a minute quantity of white matter, which is insoluble in excess of nitric acid, and becomes slate-coloured by exposure to light.

7. Heated with borax in the outer flame, a transparent bluish-green bead is obtained, which, in the inner flame, becomes turbid and brownish-red.

8. The mineral by boiling in nitric acid becomes white and then dissolves. Nitrate of silver precipitates from the solution white curdy matter, insoluble in excess of nitric acid.

Quantitative Examination.

It was impossible to separate sufficient of the mineral for analysis; it was therefore boiled with adhering matrix in successive portions of dilute nitric acid. To the filtered solution excess of nitrate of silver was added. The white precipitate, washed and dried at the temperature of incipient fusion, weighed 3·40*.

* After having detached from the filter as much of the dry chloride as

The excess of nitrate of silver having been separated by hydrochloric acid, the solution was tested with chloride of barium, but no precipitate was produced. Hydrosulphuric acid was then passed through for a considerable time. The precipitate was digested with fuming nitric acid. Sulphuric acid was added, and the whole evaporated until the excess of sulphuric acid was completely expelled. The dry product was washed with water. The insoluble white residue, washed, dried and ignited, weighed 3·90. Heated in the inner flame on charcoal with carbonate of soda, a large bead of malleable lead was obtained.

The last solution was boiled with excess of potass. The dark brown precipitate, washed, dried and ignited, weighed 0·97. Heated in the inner flame on charcoal, a bead of malleable copper was obtained.

The solution through which hydrosulphuric acid had been passed, contained iron with a minute quantity of alumina and silica.

The residual matrix, after treatment with nitric acid, was boiled with hydrochloric acid. The red-brown matter was entirely dissolved, and consisted of sesquioxide of iron. Fragments of quartz with adhering particles of metallic gold remained. The acid solution was filtered hot. On cooling, a small quantity of white matter subsided, which became slate-coloured by exposure to light, and by fusion with carbonate of soda on charcoal in the inner flame, gave a minute bead of silver.

The Results Tabulated.

Chloride of silver . . .	3·40 = Cl 0·84
Sulphate of lead . . .	3·90 = Pb 2·66
Oxide of copper . . .	0·97 = Cu 0·77

Dividing by the respective atomic weights, the chlorine, lead, and copper are found to be in the exact ratio of one equivalent of each. Thus,—

$$\frac{0·84}{36} = ·023, \quad \frac{2·66}{104} = ·025, \quad \frac{0·77}{32} = ·024.$$

A minute quantity of chloride of silver was also present.

Now the preceding results of themselves would lead to the formula ($Pb^2\,Cl + Cu^2\,Cl$) + aq; but, it must be remembered, that they merely indicate the quantitative relations between the three elements, chlorine, lead and copper. As a given weight of the mineral could not be analysed, the evidence is

possible, the former with adherent chloride was incinerated on the cover of a porcelain crucible. The residue was added to the chloride, and the whole moistened with aqua-regia and dried.

inconclusive as to the presence of oxygen. However, from an examination of the known basic chlorides of lead and copper, it appears most probable that the true composition of the mineral is represented by the following formula:—

$$(\mathrm{Pb\,Cl} + \mathrm{PbO}) + (\mathrm{Cu\,Cl} + \mathrm{CuO}) + \mathrm{aq}.$$

No salt having this composition has, so far as I am aware, been hitherto described.

XVII. *On Francolite, a supposed New Mineral.* *By* T. H. Henry, *Esq., F.R.S.**

Mr. Brooke some years since received from Mr. Nuttall some specimens of a mineral found at Wheal Franco near Tavistock, Devon, which appeared to him and to Mr. Brooke to differ in external characters from all the substances with which they were acquainted, and it was consequently provisionally named Francolite.

It consists of small masses of irregularly formed and aggregated crystals, apparently hexagonal prisms of considerable lustre, and covering the matrix with a sort of mammillated surface. It has lately been found in another mine in larger and purer specimens, and I have at Mr. Brooke's request analysed a part of one of these.

A small crystal, perfectly free from the matrix, was found by means of the blowpipe to contain lime and phosphoric acid, and a portion reduced to powder and warmed with sulphuric acid corroded glass strongly. Iron and manganese were also detected; but a very minute trace of chlorine was found.

I determined with this mineral to employ the method lately recommended by Prof. H. Rose for the quantitative separation of phosphoric acid from all bases but alumina, by means of nitric acid and metallic mercury. The crystals were reduced to powder, and freed from the matrix (quartz with copper pyrites) by treatment in a stoppered bottle with very dilute nitric acid in the cold (one part of strong acid to nineteen of water), in which the mineral was perfectly soluble. The solution was evaporated to dryness with metallic mercury in a platinum dish heated by steam (the fluoride of calcium is hereby decomposed and the fluorine dissipated), and the remainder of the process conducted scrupulously according to the directions given by Rose†.

* Communicated by the Author.

† Poggendorff's *Annalen*, March 1849; or Chemical Gazette, vol. vii. p. 202–206.

The iron and manganese were separated from the lime by ammonia after the addition of bromine.

The separation of phosphoric acid was complete; not a trace could be discovered with the bases by means of molybdate of ammonia, nor could a trace of lime be discovered by treating the $2MgO, PO^5$ with sulphuric acid, evaporating and dissolving in alcohol.

The results obtained in two analyses from crystals from different parts of the specimen were in 100 parts,—

	I.	II.
Lime	53·38	52·81
Oxides of iron and manganese	2·96	3·22
Phosphoric acid	41·34	41·80
Fluorine and loss	2·32	2·17
	100·00	100·00

This composition corresponds very nearly to that of fluor apatite, $CaFl+3(3CaO, PO^5)$, in which the lime is partially replaced by the protoxides of iron and manganese. This composition would give per cent. (see Rammelsberg's *Handwörterbuch der Mineralogie*, p. 37),—

Lime	55·88
Phosphoric acid . . .	42·02
Hydrofluoric acid . .	2·10
	100·00

And these analyses of Francolite confirm, by the direct estimation of the PO^5 by an accurate method, the results obtained by Gustave Rose in an elaborate investigation of several varieties of apatite from various localities, published many years ago*, in which the phosphoric acid was estimated from the loss.

XVIII. *On the Equation* $Q=q(w, x, y, z)=w+ix+jy+kz$. *By* William Spottiswoode, *M.A., of Balliol College, Oxford*†.

THE theorem expressed by the above equation is of considerable importance in the calculus of quaternions, and indeed essential for the application of that method to geometrical and physical problems. Sir W. R. Hamilton in his researches (Transactions of the Royal Irish Academy, vol. xxxi.), has effected the transformation by means of the symbolical division of numeral sets; but since nothing, which may throw light

* Poggendorff's *Annalen*, vol. ix.; and Berzelius, *Jahresbericht*, 1828.

† Communicated by the Author.

upon the nature and properties of these functions, is entirely without interest, I have ventured to suggest another mode in which the question may be viewed.

It is known that the symbol R_n, when prefixed to any quaternion, indicates that, out of the four constituents w, x, y, z, regarded in a definite order, that one is selected which stands in the nth place from the left-hand. If, then, to the complex symbol R_nQ there be prefixed another symbol of selection R_m, it is clear that, since in the expression R_nQ there is only one constituent from which the new selection is to be made, the combination R_mR_nQ will vanish for all values of m, except $m=1$. Hence may be formed the following symbolical system:

$$\left.\begin{array}{llll} R^2_0=R_0, & R_1R_0=0, & R_2R_0=0, & R_3R_0=0 \\ R_0R_1=R_1, & R^2_1=0, & R_2R_1=0, & R_3R_1=0 \\ R_0R_2=R_2, & R_1R_2=0, & R^2_2=0, & R_3R_2=0 \\ R_0R_3=R_3, & R_1R_3=0, & R_2R_3=0, & R^2_3=0, \end{array}\right\} \quad (1.)$$

and consequently, by the principles of the calculus,

$$Q=(R_0Q,\ R_1Q,\ R_2Q,\ R_3Q) \quad \ldots \quad (2.)$$

$$\left.\begin{array}{l} =\{(R^2_0-R^2_1-R^2_2-R^2_3)Q, \\ \quad (R_1R_0+R_0R_1+R_3R_2-R_2R_3)Q \\ \quad (R_2R_0+R_0R_2+R_1R_3-R_3R_1)Q \\ \quad (R_3R_0+R_0R_3+R_2R_1-R_1R_2)Q\} \end{array}\right\} \quad \ldots \quad (3.)$$

$$\left.\begin{array}{llll} =(R^2_0Q, & R_1R_0Q, & R_2R_0Q, & R_3R_0Q) \\ +(R_{-1}R_1Q, & R_0R_1Q, & R_{-3}R_1Q, & R_2R_1Q) \\ +(R_{-2}R_2Q, & R_3R_2Q, & R_0R_2Q, & R_{-1}R_2Q) \\ +(R_{-3}R_3Q, & R_{-2}R_3Q, & R_1R_3Q, & R_0R_3Q), \end{array}\right\} \quad (4.)$$

$$\left.\begin{array}{l} =R_{0,1,2,3}R_0Q+R_{-1,0,-3,2}R_0Q \\ \quad +R_{-2,3,0,-1}R_0Q+R_{-3,-2,1,0}R_0Q \end{array}\right\} \quad \ldots \quad (5.)$$

$$=w+ix+jy+kz \quad \ldots \quad (6.)$$

The same result might have been obtained by means of the relation

$$(w,x,y,z)=(w,0,0,0)+(0,x,0,0)+(0,0,y,0)+(0,0,0,z), \quad (7.)$$

the second side of which might be at once replaced by (4.); but in some respects the former method is preferable. In either case it appears that the expression (6.) is only one out of an infinite number which might have been obtained, by substituting for the expressions in (3.) any arbitrary combinations of R_0, R_1, R_2, R_3, respectively equivalent to those symbols themselves;

but the advantage of (6.), resulting from the relations between i, j, k, is well known.

It may be further remarked, that although

$$\left.\begin{aligned}(w, 0, 0, 0) &= R_{0,1,2,3}(w, 0, 0, 0) = Q_0 \\ (0, x, 0, 0) &= R_{-1,0,-3,2}(x, 0, 0, 0) = iQ_0' \\ (0, 0, y, 0) &= R_{-2,3,0,-1}(y, 0, 0, 0) = jQ_0'' \\ (0, 0, 0, z) &= R_{-3,-2,1,0}(z, 0, 0, 0) = kQ_0'''\end{aligned}\right\} \quad . \quad . \quad (8.)$$

(where the meanings of the expressions Q_0, Q_0', Q_0'', Q_0''' are obvious), and consequently by (7.)

$$Q = Q_0 + iQ_0' + jQ_0'' + kQ_0''' \quad . \quad . \quad . \quad . \quad (9.)$$

(an expression of the same form as (6.)); yet this would not be sufficient for the present purpose, for all the terms on the right-hand side of this equation are themselves quaternions, while the object of the transformation is to exhibit a quaternion under the form of a series of terms, which admit of being combined by laws in some degree analogous to those of ordinary algebra. That expressions of the form (9.) admit of such combinations is certainly true; but this can be proved only by means of some such formula as (6.).

As I do not propose to enter at present upon the general idea of this calculus, I will only add, that the form of the expression (3.) seems worthy of attention, from the similarity of the combinations of R_0, R_1, R_2, R_3 in it, and those of w, x, y, z,... in the squares and products of quaternions.

XIX. *Observations upon* M. Boutigny's *recent Experiment.* *By* Professor Plücker *of Bonn**.

It may perhaps be a matter of interest to you to obtain a confirmation of Boutigny's recent experiment. With his usual kindness, he exhibited to me last Easter his former experiments; and whilst admiring his rare perseverance in following up a fertile idea, I then acquired an impression that it referred to a law of nature which was by no means completely revealed, and in which opinion I was further strengthened by the report of his last experiment. In consequence of an oral communication of this experiment, M. Fessel wrote to me from Cologne, stating that on the following day he had dipped his finger into lead heated to its highest point, by

* From Poggendorff's *Annalen*, Dec. 7, 1849.

which means the projecting portion of the nail of the finger had been burnt, but in other respects the finger remained perfectly uninjured; he also stated further, that a workman in the employ of Messrs. Behren and Co., manufacturing engineers at Cologne, had made the experiment with melted iron, and would repeat it before me. I therefore accepted the offer, and, accompanied by several persons interested in the matter, proceeded to Cologne. The workman in my presence struck the unmoistened extremities of his fingers rapidly and not without fear against the surface of the iron, which had just flowed from the melting furnace into a trough, and which was afterwards used in casting a large plate for a furnace. I was thus convinced of the perfect truth of Boutigny's experiment; and whilst carefully examining the extremities of the workman's fingers, one of the two assistants of the Physical Cabinet accompanying me struck the entire surface of the open hand, which he had previously dipped in water, so strongly against the bright red surface of the iron, that some of the fused metal was ejected; the other assistant immediately afterwards also struck it with his moistened hand. After these experiments, which were made in opposition to Boutigny's precautions not to strike the mass, experiments which for the sake of precaution I wished to make before the immersion, became unnecessary; I moistened my right hand, inserted the index finger almost completely into the melted mass, and moving it very slowly through it, withdrew it in two seconds: at the same time I felt how the iron moved before my finger, but *did not experience the slightest* sensation of heat*.

I should have considered the temperature of the iron, which was about 2732° F., as below 96° F.; for on withdrawing the finger, it was not so warm as the other hand. M. Fessel also, and the other three persons who accompanied me, repeated this experiment with certain modifications: one of them with his hand dry; another remarked that the hand, after having been previously dipped in water, when withdrawn was only dry in that part which had not been immersed; a third took up the iron with the hand made hollow. The minute hairs upon the inserted fingers had entirely disappeared; but the nails were not injured, nor was any penetration of heat through the nails remarked. The hand when withdrawn had a slight

* More than twenty years ago, Prof. H. Rose, in visiting the foundries at Avestad in Sweden, saw a workman, for a small reward, take melted copper with the bare hand from a crucible and throw it against the wall. This confirms his statement, as also some other facts which Boutigny himself mentions in his memoir, that the phænomenon mentioned has long been known, especially among people engaged in the arts.—Poggendorff.

empyreumatic odour, which was stronger when there were warts upon it; but in no case was there the slightest burning sensation, or even a disagreeable sensation of heat. Hence certain minor operations in surgery might be performed with least pain by placing the foot in a bath of red-hot iron. Lastly, I made one other experiment, the result of which might have been anticipated.

I held the finger of a leathern glove, which I had well-wetted inside and had placed on a wooden rod, for nearly a minute in the melted iron; on withdrawing it, the glove was not only unburnt, but had only a temperature of about 132° F. (I had not a thermometer with me). Conjectures and theoretical views upon these remarkable phænomena would be premature without further experiments. I hope, however, soon to be able to communicate some remarks upon them.

XX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from vol. xxxv. p. 544.]

Dec. 21, 1849, THE Bakerian Lecture was delivered by Professor Graham, F.R.S., "On the Diffusion of Liquids."

The apparatus used in studying the diffusion of salts and other substances into water was very simple. It consisted of an open phial to contain the solution of the salt to be diffused, which was entirely immersed in a large jar of pure water, so that the solution in the phial communicated freely with the latter. Phials cast in a mould of the capacity of four ounces of water, or more nearly 2000 grains, were generally employed, which were ground down to a uniform height of 3·8 inches. The neck was 0·5 inch in depth, and the aperture or mouth of the phial 1·25 inch in diameter. The phial was filled up with the solution to be diffused till it reached the point of a pin dipping exactly 0·5 inch into the mouth of the bottle. This being the solution cell or bottle, and the external jar the "water-jar," the pair together form a "diffusion cell." The diffusion was stopped, generally after seven or eight days, by closing the mouth of the phial with a plate of glass, and then raising it out of the water-jar. The quantity of salt which had found its way into the water-jar—the diffusion product as it was called—was then determined by evaporating to dryness.

The characters of liquid diffusion were first examined in detail with reference to common salt.

It was found, first, that with solutions containing 1, 2, 3 and 4 per cent. of salt, the quantities which diffused out of the phials into the water of the jars, and were obtained by evaporating the latter, in a constant period of eight days, were as nearly in proportion to these numbers, as 1, 1·99, 3·01 and 4·00; and that in repetitions of

the experiments, the results did not vary more than 1-40th part. The proportion of salt which diffused out in such experiments amounted to about 1-8th of the whole.

Secondly, that the proportion of salt diffused increases with the temperature; an elevation of 80° Fahr. doubling the quantity of chloride of sodium diffused in the same time.

The diffusibility of a variety of substances was next compared, a solution of 20 parts of the substance in 100 water being always used. Some of the results were as follows, the quantities diffused being expressed in grains: chloride of sodium 58·68, sulphate of magnesia 27·42, sulphate of water 69·32, crystallized cane-sugar 26·74, starch-sugar 26·94, gum-arabic 13·24, albumen 3·03. The low diffusibility of albumen is very remarkable, and the value of this property in retaining the serous fluids within the blood-vessels at once suggests itself. It was further observed, that common salt, sugar and urea, added to the albumen under diffusion, diffused away from the latter as readily as from their aqueous solutions. Urea itself is as highly diffusible as chloride of sodium.

In comparing the diffusion of salts dissolved in 10 times their weight of water, it was found that isomorphous compounds generally had an equal diffusibility, chloride of potassium corresponding with chloride of ammonium, nitrate of potash with nitrate of ammonia, and sulphate of magnesia with sulphate of zinc. The most remarkable circumstance is that these pairs are "equi-diffusive," not for chemically equivalent quantities, but for equal weights simply. The acids differed greatly in diffusibility, nitric acid being nearly four times more diffusive than phosphoric acid; but these substances also fell into groups, nitric and hydrochloric acids appearing to be equally diffusive; so also acetic and sulphuric acids. Soluble subsalts and the ammoniated salts of the metals present a surprisingly low diffusibility; the quantities diffused in similar circumstances of the three salts, sulphate of ammonia, sulphate of copper, and the blue ammonio-sulphate of copper being very nearly as 8, 4 and 1.

When two salts are mixed in the solution-cell, they diffuse out into the water atmosphere separately and independently of each other, according to their individual diffusibilities. This is quite analogous to what happens when mixed gases are diffused into air. An important consequence is, that in liquid diffusion we have a new method of separation or analysis for many soluble bodies, quite analogous in principle to the separation of unequally volatile substances in the process of distillation. Thus, it was shown that chlorides diffuse out from sulphates and carbonates, and salts of potash from salts of soda; and that from sea-water the salts of soda diffuse out into pure water faster than the salts of magnesia. The latter circumstance was applied to explain the discordant results which have been obtained by different chemists in the analyses of the water of the Dead Sea, taken near the surface; the different salts diffusing up into the sheet of fresh water, with which the lake is periodically covered, with unequal velocity.

It was further shown that chemical decompositions may be pro-

duced by liquid diffusion; the constituents of a double salt of so much stability as common alum being separated, and the sulphate of potash diffusing in the largest proportion. In fact the diffusive force is one of great energy, and quite as capable of breaking up compounds as the unequal volatility of their constituents. Many empirical operations in the chemical arts, it was said, have their foundation in such decompositions.

Again, one salt, such as nitrate of potash, will diffuse into a solution of another salt, such as nitrate of ammonia, as rapidly as into pure water; the salts appearing mutually diffusible, as gases are known to be.

Lastly, the diffusibilities of the salts into water, like those of the gases into air, appear to be connected by simple numerical relations. These relations are best observed when dilute solutions of the salts are diffused from the solution-cell, such as 4, 2 or even 1 per cent. of salt. The quantities diffused in the same time from 4 per cent. solutions of the three salts, carbonate of potash, sulphate of potash and sulphate of ammonia, were 10·25, 10·57 and 10·51 grains respectively; and a similar approach to equality was observed in the 1, 2, and $6\frac{2}{3}$ per cent. solutions of the same salts. It also held at different temperatures. The acetate of potash appeared to coincide in diffusibility with the same group, and so did the ferrocyanide of potassium. The nitrate of potash, chlorate of potash, nitrate of ammonia, chloride of potassium and chloride of ammonium formed another equi-diffusive group. The *times* in which an equal amount of diffusion took place in these two groups appear to be as 1 for the second to 1·4142 for the first, or as 1 to the square root of 2. Now in gases, *the squares of the times* of equal diffusion are *the densities of the gases.* The relation between the sulphate of potash and nitrate of potash groups would therefore fall, to be referred to the diffusion molecule or diffusion vapour of the first group having a density represented by 2, while that of the second group is represented by 1.

The corresponding salts of soda appeared to fall into a nitrate and sulphate group also, which have the same relation to each other as the potash salts.

The relation of the salts of potash to those of soda, in times of equal diffusibility, appeared to be as the square root of 2 to the square root of 3; which gives the relation in density of their diffusion molecules, as 2 to 3. Hydrate of potash and sulphate of magnesia were less fully examined, but the first presented sensibly double the diffusibility of sulphate of potash, and four times the diffusibility of the sulphate of magnesia. If these times are all squared, the following remarkable ratios are obtained for the densities of the diffusion molecules of these different salts, each of which is the type of a class of salts, hydrate of potash 1, nitrate of potash 2, sulphate of potash 4, sulphate of magnesia 16, with nitrate of soda 3 and sulphate of soda 6.

In conclusion, it was observed, that it is these diffusion molecules of the salts which are concerned in solubility, and not the Daltonian atoms or equivalents of chemical combination; and the application was indicated of the knowledge of the diffusibilities of different substances to a proper study of endosmose.

ROYAL ASTRONOMICAL SOCIETY.

[Continued from vol. xxxv. p. 525.]

Dec. 14, 1849.—The Astronomer Royal gave an oral statement, illustrated by models and diagrams, "On the Method of observing and recording Transits, lately introduced in America; and on some other connected subjects."

The Americans of the United States, although late in the field of astronomical enterprise, have now taken up that science with their characteristic energy, and have already shown their ability to instruct their former masters. The method of observing which it is the object of this lecture to explain, was apparently suggested at first by the obvious practicability of applying the Galvanic Telegraph (so extensively used in America) to the determination of differences of terrestrial longitude; and it was first used for the differences of longitudes of the cities Louisville, Cincinnati, and Pittsburg. It appears that this first application of the principle is entirely due to Dr. Locke of Cincinnati. It became, however, evident that the same method might be used conveniently for recording the observations made at one or at several instruments in the same observatory; and Professor Mitchell, also of Cincinnati, actually prepared an apparatus at the Observatory at Cincinnati, and with it made observations, specimens of which have been sent to individuals in this country. One of these specimens was exhibited by the Astronomer Royal to the meeting. Between the systems of Dr. Locke and Professor Mitchell there is one radical difference, namely, in the principles of giving the signal of observation; in the principles of recording the signal there is also a difference, but this difference is not essential, and there appears to be no reason why the same method of making the record should not be used in both systems. It will perhaps be most convenient to begin by describing the methods of making the record, which are, or may be, common to both systems.

The general principle of the methods, as regards the act of the observer, and its difference from the act in the ordinary observation of transits, is the following:—In ordinary observations the observer listens to the beat of a clock while he views the heavenly bodies passing across the wires; and he combines the two senses of hearing and sight (usually by noticing the place of the body at each beat of the clock) in such a manner as to be enabled to compute mentally the fraction of the second when the object passes each wire, and he then writes down the time in an observing-book. In these new methods the observer has no clock near him, or at least none to which he listens; he observes with his eye the appulse of the object to the wire, and at that instant he touches an index, or key, with his finger; and this touch makes, by means of a galvanic current, an impression upon some recording apparatus (perhaps at a great distance), by which the fact and the time of the observation are registered. He writes nothing, except perhaps the name of the object observed.

The method adopted by Dr. Locke was that of interrupted indented lines, produced by the pressure of a point or style (effected by a gal-

vanic magnet whose current is interrupted occasionally) upon a fillet of paper, which is drawn by machinery with a nearly uniform speed under the style. With this apparatus it is evident that a signal almost instantaneous in duration can be given; either by supposing the ordinary state of the apparatus to be that of exerting a pressure on the style, and giving the signal by interrupting the galvanic current; or by supposing that in the ordinary state of the apparatus no pressure is exerted, and that the signal is given by completing the galvanic circuit, which animates the magnet and creates an instantaneous pressure. In the first of these forms the record will exhibit the appearance of long lines interrupted by very short intervals; in the second it will have the appearance of punctured dots, with wide spaces between them. Now, as no reliance whatever can be placed on the uniformity of speed of the machinery which draws the fillet, it is necessary to make such connexion of the apparatus with a trustworthy clock, that the seconds (or other uniform intervals) of the clock shall be recorded in the same manner upon the same fillet. If this be effected by the same galvanic communication and the same magnet, then, in the first case (or that in which the ordinary state is that of pressure on the style), the record will consist of an indented line, interrupted at short and sensibly equal intervals corresponding to the seconds marked by the clock, and interrupted also at other points corresponding to those instants at which the observer has, by a voluntary effort, interrupted the galvanic circuit in order to give a signal. This is the method adopted by Dr. Locke. In the second case (where in the ordinary state no pressure is exerted), there will be a series of punctured dots at sensibly equal intervals corresponding to the seconds of the clock, and, mingled with these, there will be the dots corresponding to the instants at which the observer has completed the galvanic circuit. This is the method adopted by Professor Mitchell.

In either of these methods there is no difficulty in producing such a variation, repetition, or omission, of the clock-signals, as will distinctly mark the beginnings of minutes, of every five minutes, &c.

Dr. Locke appears to have been induced to adopt the method of registering by interrupting the circuit (which in its ordinary state was complete), by the following consideration:—His object was to record upon one moving fillet of paper at Cincinnati the observations made at three stations (Louisville, Cincinnati, and Pittsburg), and also the seconds of the clock. He remarked, then, that the interruption of the circuit for any one place would interrupt it for all; and therefore, at whatever place the signal of observation was made by interruption of circuit, the corresponding interruption would necessarily be made in the indented line on the fillet of paper. But he remarked also that the connexion of the parts of the circuit at one place would not effect the connexion for all; and he appears to have inferred that, if a single wire only were used extending along all the stations, it would be impossible to use it by the method of completing the circuit for signal, and that, in fact, a separate wire would be necessary for each separate station. This inference, as the Astronomer

Royal showed by diagrams, is incorrect. If the battery and the recording apparatus are near together, then it is only necessary to have a single trunk-wire passing by all the stations (including the clock as one); and to have a branch-wire passing through the recording apparatus to the battery, and a communication from the other end of the battery to the ground; and also to have a branch-wire through each station to the ground, interrupted in its ordinary state, but admitting of connexion at the will of the observer, or by the motion of the clock. Then, at whatever place the observer completes the connexion with the ground, the galvanic circuit through the recording apparatus is completed. Or, in the most unfavourable case (which is never likely to occur), of the battery at one extreme station, and the recording apparatus at the other extreme station, it is only necessary to have one trunk-wire proceeding from the battery through all the stations, and another trunk-wire parallel to the first proceeding from the recording apparatus through all the stations; then, at whatever station the connexion between these two trunk-wires (properly furnished with small branch-wires) is made by the observer, the circuit through the recording apparatus is completed.

The method of recording by the use of a fillet of paper, strictly speaking, is used only by Dr. Locke. The equivalent method used by Professor Mitchell is, to cause a circular disc to revolve with a smooth and nearly uniform motion (by means of a Fraunhofer's regulator), upon which disc the impressions of the style form a dotted circle; then, at the end of each revolution, a tooth upon the axis of the disc takes hold of a fixed rack and moves the travelling frame, which carries the centre of the disc, through a small space, so that the traces of the succeeding circle are prevented from mixing with those of the preceding circle. Professor Mitchell alludes to the practicability of using a cylinder which turns upon a screw axis, so that the traces will be made in a perpetual spiral; and there can be little doubt that this construction would be preferable to that of the circular disc. Among other reasons, the habitual estimation, or the measure, of the fractions of the seconds, would probably be more accurate where the length corresponding to a second is uniform than where it varies from one circle to another, as it does on the circular disc.

The momentary interruption of the circuit in Dr. Locke's method, or the momentary completion in Professor Mitchell's method, for the register of observations, is made by touching a key nearly similar to that of a musical instrument. This key may be attached to the observing chair or to the astronomical instrument. Dr. Locke has not, however, fully described the mechanism by which he makes his clock interrupt the circuit. Professor Mitchell describes the action of his clock in the following manner:—A delicate fibre attached to the pendulum of the clock acts upon a "cruciform" lever [probably a rectangular or "bell-crank" lever], and thus, in every double swing of the pendulum, allows a metallic point to dip into a cup of quicksilver, and to complete the galvanic circuit. Thus, in Professor Mitchell's register, the clock-dots are made at every two seconds.

Professor Mitchell has found it convenient to use, for the seconds-record, a pen or style different from that which is used for the observation-record. He does not mention whether he uses for the seconds-record a battery different from that used for the observation-record; but this appears necessary to prevent the confusion which might arise in the register on making observations exactly at the second of time; for the completion of the battery-circuit by one wire would probably interfere with the efficiency of the circuit through the other wire.

The Astronomer Royal does not hesitate to express his preference of Professor Mitchell's form of register to Dr. Locke's.

The practicability of this method of recording observations being fully established, it then becomes an important question for the observing astronomer, whether this method is or is not more accurate than the usual method of observing by the combination of eye and ear. The question is, really, whether the connexion between the nerves of the eye and of the finger is or is not closer than that between the nerves of the eye and of the ear: it is purely a physiological question, which can be settled only by experience. Professor Mitchell has investigated it in the following manner. Extracting from the printed Greenwich Observations a number of transits of α Coronæ and comparing their intervals with the intervals as established from observations of the pole-star, he has obtained a measure of what may be called Greenwich Irregularities. Using the same process for the observations recorded by the Galvanic Register, he has obtained a measure of Cincinnati Irregularities. The magnitude of the latter is only about one-fourth of that of the former. The Astronomer Royal suggested that a portion of this difference might be owing to the difference in the state of the two atmospheres, the atmosphere of England being perhaps comparatively unfavourable to accurate observation. The result of Professor Mitchell's comparison is, however, very encouraging as regards the probable success of the method.

One important advantage of this method would be the contraction of the time of observing a transit. Instead of using wires 12^s or 15^s apart, intervals of 2^s will be amply sufficient. Advantage may be taken of this, either for the observation of numerous objects, or for observation over numerous wires. One inconvenience of the method, however, is, the trouble of translating the graphical registers into numbers. Another inconvenience, of great weight, is the extent of recording surface that will be required, unless the recording machinery be very frequently disengaged, and its records (to a certain degree, not essentially injurious, but troublesome) be rendered discontinuous. In the Royal Observatory of Greenwich it is by no means uncommon to have trains of interrupted observations extending over twelve hours, or even a longer time, at once. An unbroken series of time-marks of 12 hours in Dr. Locke's method, allowing 1 inch to each second, would require 3600 feet of paper fillet; in Professor Mitchell's method, allowing $\frac{1}{3}$ inch for each second, and $\frac{1}{10}$ inch between successive lines of dots, it would require a sheet of 1440 square inches of paper.

The Astronomer Royal then stated that the possible advantages of this method appeared so great that he had begun to contemplate the practicability of adopting it in the Royal Observatory. One reason exists there which probably exists nowhere else, namely, the regularity of use of the Altitude and Azimuth Instrument by the method of transits, and the necessity of referring these transits (with the smallest possible uncertainty on personal equation) to the same clock to which the meridional transits are referred. In adopting Professor Mitchell's general method, he would propose to record the observations upon a cylinder, perhaps revolving upon a screw-axis. It is proper to remark, that this screw-axis may be so mounted upon friction-wheels that the friction would be quite imperceptible. The friction-wheels must not have their planes transverse to the axis, but in such a position that their edges follow the threads of the screw; and, supposing that there are two friction-wheels at each end, one of the four must be fixed in place, and the others must be mounted on frames which have a small hinge-motion in the direction perpendicular to the planes of the wheels. Or, instead of using the screw-motion, the frame which supports the axis of the cylinder may run upon a railway, along which it will be carried by rack-and-pinion mechanism, receiving its movement from the clock-work. The motion of the cylinder may be given by a toothed wheel on one end, which works in a stiff pinion, long enough to admit the toothed wheel to slide along it through a space equal to the length of the cylinder.

In using this cylindrical record, it is obvious that great convenience would be gained if the movement of the cylinder could be made so perfectly uniform that it could be adopted as the transit-clock. Then the second-registers could be made on it by the same clock which moves it (either by galvanic contact or mechanically), and their places would bear a constant relation to the lines parallel to the axis of the cylinder. The Astronomer Royal, therefore, urged strongly the importance of improvements of the centrifugal or conical-pendulum clock, as the only instrument yet made which is able to do heavy work with smooth motion, and with an accuracy at present so great as to make it probable that, with due modification, the greatest accuracy may be obtained. Setting aside the consideration of Fraunhofer's clock, as an instrument which, for purposes like these, is rude, the Astronomer Royal stated that, as he believed, the first efficient conical-pendulum clock was that made by Mr. Sheepshanks, in which the expansion of the balls to a certain angle produces suddenly a friction which, as soon as it amounts to an equivalent to the maintaining power, prevents further acceleration. Theoretically, the speed of this clock depends in a very small degree on the maintaining power. In order to remedy this small defect, the Astronomer Royal had introduced the use of water power (by the modern form of Barker's mill or reaction engine) as the moving force, and had regulated the supply of water by Sieman's chronometric governor. The principle of this governor may be used in various forms; the following, perhaps, will serve to explain it most clearly.

Suppose the last wheel of the moving-train to be a beveled wheel whose plane is horizontal, carried by a vertical spindle; and suppose the spindle of the centrifugal balls to be above the moving-train spindle, in the same vertical line, but separate, and carrying a beveled wheel similar to the other, but in the opposite position; and suppose the connexion between these two beveled wheels to be made by an intermediate beveled wheel, whose plane is vertical, and whose axis passes between the two; the train-spindle and the ball-spindle will evidently rotate in opposite directions. The axis of the intermediate wheel must not be fixed, but must be capable of turning in a horizontal plane, and it must be pulled by a weight (acting by a line over a pulley, or a right-angled lever), in the direction of turning the ball-spindle; it must also be connected with the water-valve in such a way that the same pull will tend to open the water-valve. It is evident now that, while the axis of the intermediate wheel is free to move, the force driving the beveled wheel of the ball-spindle is rigorously uniform, and is equal to half the weight which pulls the axis of the intermediate wheel. If the moving power is suddenly increased, the immediate effect of this increase is, not to accelerate the ball-spindle, but to drive the intermediate wheel in such a manner that its axis lifts the pulling-weight, and partially closes the water-valve. An opposite motion takes place if the moving power is suddenly diminished. In this manner the water-valve is maintained in that state which supplies the force that exactly produces a determinate moving power upon the ball-spindle.

In some localities it may be difficult to obtain the proper supply of water, and in some there may be danger of obstruction by frost. The Astronomer Royal had, therefore, endeavoured to effect the same object by the use of weights. By a remontoir-train the uniformity of rotation of the balls might undoubtedly be secured; but then the motion of the primary wheels of the train would be intermittent. The following plan, however, appears to the Astronomer Royal to be perfect. Let the arrangement of the three beveled wheels and the pulling-weight be exactly the same as that above described, but let the train spindle carry a broad flat disc, whose plane is horizontal: upon this let a lever press by a definite projection from its lower side, and on the upper side of the lever let there be a sliding weight (as a ball running in a groove); let the spindle of the intermediate beveled wheel command the place of this ball by a fork projecting downwards, loosely including it; and let the tendency of the pulling weight be to draw this ball towards or over the fulcrum of the lever, so as to diminish the pressure of its definite projection upon the flat disc. Then, if the moving force is suddenly increased, its immediate effect will be to drive the intermediate wheel in such a direction as to push the running ball further along the lever, and thus to increase the pressure of its projection upon the flat revolving disc, and to produce a friction which will absorb the excess of power.

There are still two practical defects which it is desirable to remove.

The first is, the amount of friction in the rotation of the balls, which, indeed, is so great, that in the clock-work used for equatoreal motion it consumes by far the greater part of the power. In the shape in which Sieman's regulator is usually applied to steam-engines, with one ball revolving by a rod whose support is a ball-and-socket joint, the friction is intolerably great. When balls are mounted on a vertical spindle, the friction, although much diminished, is still far too great. The Astronomer Royal had therefore directed his thoughts to the mounting of the conical pendulum, so as greatly to diminish the friction. In the summer of 1846 he chanced to see at the *Gewerbe Aufstellung* of Wiesbaden* a beautiful centrifugal-ball clock, intended for a drawing-room. The maker had acutely remarked that the circular motion might be resolved into two rectangular motions, and that each of these might be produced by the vibratory motion of a knife-edge. The pendulum, therefore, vibrated immediately by a knife-edge upon concave agates carried by a small frame, and this frame itself was furnished with knife-edges in the direction transversal to the former, which vibrated upon concave agates carried by the fixed frame of the clock. The pendulum thus moved with so much freedom that it was kept in conical vibration by a small maintaining power, acting ultimately by a light radial arm to maintain the rotation. The arc of expansion was determined (as in ordinary pendulums) merely by the resistance of the air.

This motion on knife-edges is liable to the same objections as the use of knife-edges for clock-pendulums. The Astronomer Royal therefore proposes to substitute for them a mounting by springs. The pendulum is immediately supported by two springs from a frame, and this frame is itself supported from the fixed parts of the clock-frame by two springs whose relative position is transversal to that of the former, and whose plane of vibration is transversal to that of the former; the form of the intermediate frame being such that, when the pendulum hangs in a vertical position, the upper ends of the four springs will be in one horizontal plane, and the lower ends will also be in one horizontal plane. A model of this mounting was exhibited. The motion is most satisfactory; the pendulum revolves many times before the diameter of its circle is diminished to one-half. In the complete clock-work the power of the train is to act on this by a radial arm.

The second defect, which will only be sensible when the mechanism has received these improvements (but of the distinct effect of which the Astronomer Royal has no doubt), is the want of compensation for the thermal expansion of the pendulum rod. Supposing the limiting arc of vibration determined by rotation within a ring,

* The mechanical talent of inland Germany, having had little employment in the construction of powerful engines or manufacturing machinery, appears to have developed itself in the invention of clock-work. The Museum in the Schloss of Gotha contains a remarkable collection of clocks embodying every conceivable device. None, however, has appeared to the Astronomer Royal so remarkable for the accuracy of its mechanical conception as that which is described in the text.

the contact of the rod with which produces the friction that prevents further acceleration, the Astronomer Royal proposes the following simple construction. The ring is to be of brass, and is to be carried in two or more points of its circumference by horizontal bars, each bar being supported at the extremity furthest from the ring by an iron pillar, and at some point between the iron pillar and the ring by a brass pillar; the pendulum rod and all other parts of the frame being of iron. The expansion of the brass ring, and its elevation by the effect of the different expansion of the two pillars carrying each horizontal bar, will both contribute (and may be made to do so to any assignable degeee) to permit the greater angular expansion of the conical motion which is necessary for isochronism when the pendulum rod is lengthened by heat. [It is well known that the time of rotation of the conical pendulum depends only on the vertical depression of the ball below the horizontal plane passing through the centre of motion.]

It may, however, be thought preferable to employ a mercurial pendulum of the ordinary construction as regards compensation, revolving in a very small circle, whose diameter is perhaps equal to the usual arc of vibration of a common clock pendulum, and whose dimensions are limited only by the resistance of the air. The radial arm ought to act on a slender spike at the bottom of the mercury-cistern. The pulling-weight would be very light, but the efficiency of the regulation would not be diminished by that circumstance. No ring would be necessary, except as a safety-guard, to prevent the machinery from running wild on any accidental excursion of the pendulum beyond the end of the radial arm.

The Astronomer Royal then remarked that, considering the problem of smooth and accurate motion as being now much nearer to its solution than it had formerly been, it might be a question whether, supposing a sidereal clock made on these principles to be mounted at the Royal Observatory, it should be used in communicating motion to a solar clock. It might by some persons be thought advantageous, even now, that the drop of the signal-ball (at 1^{h} Greenwich mean solar time) should be effected by clock-machinery; and it is quite within possibility that a time-signal may be sent from the Royal Observatory to different parts of the kingdom at certain mean solar hours every day, by a galvanic current regulated by clock-machinery. Whether it would be advisable that this should be done by machinery proceeding originally from the sidereal mover, would be a question for consideration at the proper time; but, at all events, the Astronomer Royal desired to show that the problem is practically possible to an astonishing degree of accuracy. Dr. Henderson, of Newferry near Birkenhead, had communicated to the Astronomer Royal, and had permitted him to make known to the Society, the following numbers for the teeth of wheels. If there be three spindles, Nos. 1, 2 and 3, No. 1 revolving in a mean solar day of 24 hours, or 86,400 solar seconds, and if No. 1 carries a wheel of 247 teeth working in a wheel of 331 teeth on No. 2, and if No. 2 also carries a wheel of 43 teeth working in a wheel of 32 teeth on No. 3, then No. 3 will revolve in $23^{h}\ 56^{m}\ 4^{s}{\cdot}09001$. Again, if there be

four spindles, Nos. 1, 2, 3 and 4, No. 1 revolving in a mean solar day of 24 hours, and if No. 1 carries a wheel of 96 teeth working in a wheel of 79 teeth on No. 2, and if No. 2 also carries a wheel of 157 teeth working in a wheel of 133 teeth on No. 3, and if No. 3 also carries a wheel of 72 teeth working in a wheel of 103 teeth on No. 4, then No. 4 will revolve in $23^h\ 56^m\ 4^s{\cdot}09235$. The length of the sidereal day adopted in the Nautical Almanac is $23^h\ 56^m\ 4^s{\cdot}0906$. The approximations to it obtained above are very remarkable. By reversing the same train of wheels, accurate motion corresponding to sidereal time will be made to generate motion corresponding with the same degree of approximation to mean solar time.

A Method of Correcting the Errors due to the Forms of the Pivots of a Transit Instrument. By Professor Challis.

The method proposed by the author requires the solution of the following problem:—Having drawn from a selected point of the middle wire of the telescope, situated near where the transits are usually taken, a straight line to the optical centre of the object-glass, it is required to find, independently of the forms of the pivots, the small angle made by this line with the plane of the meridian for any position of the telescope. The solution of this problem may be effected by employing Bohnenberger's collimating eye-piece in conjunction with the method of measuring by micrometer-microscopes the positions of two dots at the extremities of the pivots, first suggested by the Astronomer Royal and applied by him in testing the forms of the pivots of the new altitude and azimuth instrument at the Greenwich Observatory. The collimating eye-piece gives the angle (α) which the line of collimation makes with the meridian plane when the telescope is directed to the nadir. If $y_1\ y_2$ be the vertical microscope readings for bisections of the dots in this position of the telescope, $y'_1\ y'_2$ be the vertical microscope readings, and $x'_1\ x'_2$ the horizontal microscope readings for bisections of the dots, when the telescope is pointed to an angular distance, z, from the zenith southward, and ξ be the small angle required, then

$$\xi = \alpha + \frac{y_2 + k - y_1}{\mathrm{D}} + \frac{y'_2 + k - y'_1}{\mathrm{D}} \cos z + \frac{x'_1 - x'_2 - h}{\mathrm{D}} \sin z,$$

D being the distance between the dots, and h and k certain constants, by means of which the readings of the two microscopes are referred to the same vertical and horizontal planes. The constant k is found by measuring the difference of altitude of the dots in reversed positions of the instrument, the telescope being directed to the nadir, and at the same time noting by the collimating eye-piece the change of inclination of the line of collimation to the vertical caused by the reversion. The correction to be applied to an observed time of transit across the middle wire to reduce it to the time of meridian transit is $\frac{\xi}{15} \times$ cosec N.P.D. Hence h may be found by two consecutive transits of Polaris. These constants being known, the angle ξ may be calculated for zenith distances separated by intervals of 5°, and extending from $-90°$ to $+90°$. The differences between these values of ξ and the values of the same angle which

would be obtained *at the same time*, in the usual manner, on the supposition that the pivots are perfectly cylindrical, are certain functions of z, which may be assumed to remain constant throughout the year. A table of these differences multiplied by $\frac{\text{cosec N.P.D.}}{15}$ being formed, in which the argument is the north polar distance, any observed time of transit is first to be corrected by a quantity derived by interpolation from this table, and then the remaining reduction to meridian transit may be effected, by obtaining the collimation, level, and azimuth errors, and applying corrections on account of them, by the ordinary processes.

XXI. *Intelligence and Miscellaneous Articles.*

DR. BIALLOBLOTZKY'S JOURNEY TO DISCOVER THE SOURCES OF THE NILE.

DR. BEKE has recently addressed to the subscribers to Dr. Bialloblotzky's projected exploratory journey into Eastern Africa, a circular letter, announcing that he has remitted to that traveller, who is now in Egypt, the funds necessary for enabling him to return home, and submitting to them a general and final statement of the sums received by him and of their appropriation.

Dr. Beke adds that the labours of the Church Missonaries stationed at Rabbai Empia, near Mombás, seem likely to result in the realization of the views as to the geography of Eastern Africa, which were enunciated by him in the year 1846, and which Dr. Bialloblotzky's expedition was intended to verify.

Already has the Rev. Mr. Rebmann, in his several exploratory journeys, discovered, in about 3° 40′ S. lat. and 36° E. long., a lofty mountain, named Kilimandjáro, whose summit is covered with perpetual snow, and obtained information respecting a region further in the interior, called Uniamési, or "*the country of the Moon*;" and he has further ascertained the existence, in Uniamési, of a large lake, which is *not* (as has been supposed) identical with *Nyássi* or "the Sea"—the great lake of Southern Africa, commonly known as lake Marávi—but from its name, Usámbiro, is apparently the Lake Zambre of the Portuguese of the 16th and 17th centuries.

On the other hand, the Egyptian expeditions for exploring the Upper Nile have ascended the river as far as the fourth parallel of north latitude, where they have found it to be still a very large stream, about 2000 feet in breadth during the rains; and as the country of Uniamési (or Mono-Moëzi) may be approximatively placed in 2° to 4° S. lat. and 29° to 34° E. long., the head of the Nile would, by its course being extended only 300 or 400 miles beyond the extreme point reached by the Egyptian expeditions, be brought near if not into this country of Uniamési.

Should it really be the case that the Nile rises in the snow-capped Kilimandjáro or other similar mountains, in the vicinity of the lake in "the country of the Moon," the fact would be almost literally in

accordance with the assertions made 1700 years ago by the geographer Claudius Ptolemy of Alexandria, that the sources of the Nile are in the Mountains of the Moon, and that the lakes of that river receive the snows of those mountains.

According to the latest intelligence received from the Missionaries, Mr. Rebmann had set out on the 5th of April last for Uniamési and the lake there; so that we may confidently anticipate the speedy solution of the great problem of geography—*Nili quærere caput.*

ON THE ESTIMATION OF THE WEIGHT OF ANTIMONY. BY M. H. ROSE.

Antimonious acid (oxide of antimony $Sb+3O$) may be estimated in a mode analogous to arsenious acid, by means of a solution of gold. This method requires, however, more precautions than the determination of arsenious acid. For, at the same time that the gold is reduced, antimonic acid is separated, which when it has once been precipitated, is very difficultly soluble in hydrochloric acid; this precipitation must therefore be prevented by the previous addition of a great excess of hydrochloric acid.

Separation of Antimony from Tin.—The author has already proposed a method for the separation of these two metals: this consists in eliminating the antimony in the form of antimoniate of soda by means of water, from a solution of tin in hydrate of soda. As, however, antimoniate of soda is not perfectly insoluble in water, this method does not give perfectly accurate results; a more precise one consists in the separation of antimoniate of soda from stannate of soda by means of dilute alcohol.

When an alloy of these two metals is to be separated, the first step to be taken, is that of completely oxidizing both by nitric acid. The acids obtained, from which the nitric acid is to be expelled by gently heating them, are to be fused in a silver crucible with hydrate of soda. The fused mass is moistened with water, and then alcohol is to be added; dilute alcohol is to be used for washing the antimoniate of soda.

The alcoholic solution of stannate of soda is exposed to a gentle heat to expel the greater part of the alcohol, then diluted with water, and saturated with dilute sulphuric acid, and lastly the tin is to be precipitated in the state of sulphuret by means of hydrosulphuric acid, and then to be converted into oxide of tin.

The antimoniate of soda is dissolved in a mixture of hydrochloric and tartaric acids, and the antimony is precipitated from the solution by hydrosulphuric acid gas in the form of sulphuret of antimony.

Separation of Antimony from Tin and Arsenic.—The metals are oxidized by nitric acid, the oxidized mass is evaporated to dryness, and then fused in a silver crucible with hydrate of soda. The fused mass is diffused in water and treated with dilute alcohol. Antimoniate of soda remains, which does not dissolve, washed with dilute alcohol, and is afterwards dissolved in a mixture of hydrochloric and

tartaric acids; from this solution the antimony is precipitated in the form of sulphuret by hydrosulphuric acid gas.

It is hardly necessary to expel by heat the alcohol from the solution from which the antimoniate of soda has been separated; it is to be saturated with hydrochloric acid, and without filtering the arseniate of tin which is formed, a current of hydrosulphuric acid gas is to be passed into it.

Separation of Antimonious and Antimonic Acids.—These two acids may be quantitatively determined when they occur together in solution in hydrochloric acid, by a solution of gold. This solution of gold is a good substance for discovering the existence of antimonious acid in the presence of a great excess of antimonic acid; but solution of nitrate of silver is a still more sensible reagent for discovering antimonious acid. If to a solution of antimonic acid in hydrate of potash, a solution of nitrate of silver be added, an intense black precipitate is obtained. This precipitate is not soluble in ammonia; but it takes this substance from oxide of silver which has been precipitated by an excess of solution of potash; when a solution of nitrate of silver is added to one of antimoniate of potash, a white precipitate of antimoniate of silver is obtained, with a yellowish tint. If the solution contains free potash, the precipitate of oxide of silver, simultaneously formed, is brown. The two precipitates are completely insoluble in ammonia. If, on the other hand, the solution contains but a small quantity of antimonious acid, there remains, when the precipitate is treated with ammonia, a black precipitate which does not dissolve.—*L'Institut*, Decembre 26, 1849.

ON THE ASSOCIATION OF SILVER WITH METALLIC MINERALS, AND METHODS OF EXTRACTING IT. BY MM. MALAGUTI AND DUROCHER.

In a former memoir the authors showed that silver exists in many metallic sulphurets, in which its presence had not been suspected, and they are now able to state that most of them contain silver, even when not coming from situations in which this metal is extracted. Thus of more than two hundred specimens examined, only about one-twentieth contained no silver. Many indeed contained traces only, and there would have been some uncertainty if the usual methods of assaying had not been modified.

It was soon discovered that the humid process is totally inapplicable in such researches; litharge was then prepared almost without silver, and the purity of the fluxes and the other reagents employed, was ascertained. Afterwards the conditions under which the fusions should be performed, so as to lose as little as possible, were determined, and they ascertained that buttons of silver, which weighed the sixteenth of a milligramme, did not disappear in cupellation, even when allied with 30 grammes of lead.

In the experiments performed on the roasting of various sulphurets, the authors were surprised on finding that silver contained in blendes would lose more than half by sublimation. In certain cases this metal

therefore volatilizes much more readily than was supposed; it incrusted the surfaces of the apparatus employed; the same happens to the silver in roasting galenas; this fact explains an important metallurgic fact, which is, that notwithstanding the precautions taken to collect pulverulent cadmium in the condensation chambers, there is always a considerable loss of silver which is carried up, and fixed on the surface of the apparatus, that it cannot be detached; this was shown to be the case by experiments.

Silver is unequally diffused in the various metallic compounds: thus oxides and saline combinations are always poorer than sulphurets, and among the latter, the compounds with a radical of iron are generally less rich in silver than those of lead, copper and zinc. These remarks on the unequal distribution of silver in natural substances, are moreover confirmed by what passes in the operations in the dry way, whether performed in the laboratory or metallurgic establishments.

The universal diffusion of silver in the mineral kingdom induces the belief that other metals are perhaps as widely disseminated in nature; this is already known to be the case with iron. With this view the authors examined crystalline minerals possessing all the characters of purity. Twelve specimens of galena, besides silver, contained very sensible quantities of iron, copper and zinc.

In order to ascertain the state in which silver is associated in small quantity in various metallic minerals, and especially in sulphurets, sulphoarseniurets, and sulphoantimoniurets, such reagents were first employed as were supposed capable of acting upon metallic silver, and not upon its sulphuret, especially when it is combined with other metallic sulphurets. Neither liquid chlorine, bichloride of copper, nor persulphate of iron gave very positive results; mercury yielded more precise indications: of thirty-eight specimens operated upon, and of which some were considerably rich, eleven only yielded to mercury a part of their silver. The comparison of results deduced from experiments made under similar conditions upon substances into which metallic silver or its sulphuret had been in various ways introduced, led to the conclusion that the silver, probably, does not exist in the same state in all sulphurets, containing small quantities of it, but that it is most frequently combined in the state of sulphuret with the substance which it accompanies.

The authors have completed their preceding experiments, demonstrating that metallic sulphurets cannot contain silver in the state of chloride or bromide; and they have noticed some remarkable reactions occurring between chlorides and sulphurets. The authors divide these into three groups:—1st, bimolecular sulphurets, such as those of zinc, cadmium, lead, &c.; 2ndly, sulphurets possessing several molecules of sulphur, and capable of parting with some of it, bisulphuret of tin for example; 3rdly, sulphurets not saturated with sulphur and susceptible of combining with it, such as the protosulphuret of copper.

The first react upon the chloride of silver by double decomposition; the second undergo partial reduction, becoming protosulphuret; the

last partly reduce the chloride of silver, and also act upon it by double decomposition.

The arseniurets, sulpharseniurets and sulphantimoniurets, placed in the same circumstances, produce upon the chloride of silver an action resembling that of the sulphurets.

These different bodies were added to chloride of silver dissolved in ammonia, and sometimes in hydrosulphite of soda; but it was found that the presence of the solvent produced no other effect than that of accelerating the phænomenon, and rendering observation of it more commodious, but it did not alter the essential conditions of it.

It is remarkable to observe that the decomposition produced by the sulphurets, arseniurets, &c., is often as complete as if the operation was conducted on bodies dissolved in water. As examples of this may be cited, native sulphuret of copper, arseniuret of antimony, arsenical cobalt, arsenical nickel, &c. Certain sulphurets, though but few, do not act; such for example are the sulphuret of mercury and gray cobalt, which in this respect differs much from gray nickel. Metallic iron resembles it in this respect, that it does not precipitate, or but very slightly, silver from solution in the form of concentrated ammoniacal chloride, or even in the form of nitrate.

The power of sulphurets to decompose chloride of silver is generally more marked in those which act by way of reduction than in those which produce double decomposition; moreover this power appears to have relation to the electro-chemical state of the metals. It must also be added, that various minerals belonging to the same species possess decomposing powers varying according to their different composition, crystalline form, density and cohesion.

Bromide of silver, put into contact with metallic sulphurets, offer the same phænomena of decomposition as the chloride. In short, all these facts appear to depend upon a general law of the reactions of the sulphurets on the chlorides, and of insoluble on soluble salts. Moreover, the authors find that these reactions are produced in the dry as well as in the humid way: thus galena decomposes chloride of silver in fusion; blende was found to detain the vapour of this chloride and to convert it into sulphuret of silver. The same vapour is also decomposed with the assistance of heat by quartz, felspar, argil and silicates in general.

The reactions of sulphurets on chlorides, produced under such various circumstances, evidently possess a general character, and the observation of various metalliferous deposits offers additional confirmation of it; for the chloride and bromide of silver do not occur among the same metallic sulphurets, but in the upper parts of veins, which have been altered and oxidized by the influence of external causes. The authors also deduce from their experiments, the explanation of certain geological phænomena; for example, the concentration which the mineral of native and sulphuret of silver of the veins of Konigsberg has undergone; a mineral which occurs agglomerated by schistose bands impregnated with various metallic sulphurets, as iron and copper pyrites, blende and galena.—*Comptes Rendus*, Decembre 10, 1849.

ON THE PRESENCE OF LEAD, COPPER, AND SILVER IN SEA-WATER, AND OF THE LAST METAL IN ORGANIZED BEINGS. BY MM. MALAGUTI, DUROCHER AND SARSEAU.

The authors decided on making researches on the above subject by the fact, to which two of them had already drawn the attention of the Academy, that silver is widely diffused in metallic minerals. For example, in blendes and pyrites its presence is very common; its absence from galenas is an exception. But as salt water by long action is capable of converting these substances into chlorides, which it dissolves, the author inquired whether sea-water might not contain these metals, which in the form of sulphurets it either washes or covers in the earth. Such were the motives for research: they were not, however, undertaken till after having dispelled every kind of fallacy, by a minute examination of the reagents and recipients to be employed.

The authors proved the presence of silver in the water of the ocean by two different methods; it was taken at a distance of some leagues from the coast of St. Malo, and the results were compared by researches as to this metal in the fucus of the same latitude. Of all those which the authors examined, the *F. serratus* and *ceramoides* were the richest; their ash contained at least $\frac{1}{100,000}$, whilst the sea-water contained but a little more than $\frac{1}{100,000,000}$.

If the water of the sea is argentiferous, the common salt and artificial products derived from it ought also to be so; and, in fact, experiment proved this to be the case. Common salt, common muriatic acid, and artificial soda contain minute quantities of silver. Does, however, the generality of the fact depend upon a constant law, or on a collection of variable causes?

The authors sought to solve this question by examining the salgem of Lorraine, which very probably represents the ancient seas: it was fortunately found to contain silver, and hence the authors conclude that the presence of this metal in the waters of the ocean depends upon a constant law.

Never losing sight of the point from which they set out, the authors inquired whether terrestrial plants might not assimilate, by means of their roots, the silver which in the state of solution might be presented to them by subterranean waters. This water, mineralized by many salts, and among others by chlorides, would be enriched with silver by its action on the metallic sulphurets with which it would meet in its course. The examination of ashes derived from a mixed heap of different kinds left no doubt of the presence of silver in different vegetable tissues; this fact indicated another, that of the presence of this same metal in the animal œconomy. This the authors conceive themselves able to state, from experimenting on considerable quantities of ox blood.

Lastly, it remained to be determined whether additional evidence of the extreme diffusion of silver, and its independence of every accidental or inherent cause in the modern world, could not be obtained

by researches on ancient vegetables. The ashes of coal were examined, and the authors state that the presence of silver in them was not so satisfactorily demonstrated as in the ashes of modern vegetables.

After many useless attempts, the authors gave up the direct search for lead and copper in sea-water; but they feel convinced that it is to be found. By examining several species of fucus, they found in their ashes $\frac{18}{1,000,000}$ of lead and a little copper: this proves that if the quantity of these two metals in sea-water is so minute as to escape detection by reagents, it is not sufficiently so to escape the assimilating power of plants.

To recapitulate the principal facts to which the authors call the attention of the Academy; they are the presence of silver in sea-water, in sal-gem, and in organized beings; the presence of lead and copper in certain species of fucus, and consequently in the medium in which these plants live.—*Comptes Rendus*, Decembre 26, 1849.

OBSERVATIONS ON THE ESTIMATION OF LIME. BY M. ALVARO REYNOSO.

Lime is one of the substances which chemists have occasion most frequently to estimate the quantity of; and as this is always effected in the state of oxalate of lime, it is important to know all the properties of this salt, in order to avoid the errors which analysis would otherwise occasion.

With this view the author has undertaken some experiments on the action of soluble salts, capable of becoming insoluble oxalates by reacting on oxalate of lime.

Oxalate of lime is converted by the influence of a soluble salt of copper, as the chloride, sulphate and nitrate, into oxalate of copper. The presence of lime has been ascertained in the liquor separated from the oxalate of copper formed. The precipitate formed by mixing one equivalent of chloride of calcium with one equivalent of oxalate of ammonia dissolves in chloride of copper when poured into it at once; but by long standing, agitation, or boiling the liquor, a precipitate is formed, which is shown by analysis to be oxalate of copper; when the chloride of copper is gradually added, the oxalate of lime is converted into oxalate of copper, and there is at the same time formed a soluble salt of lime: the oxalate of copper precipitates, and is no longer redissolved by an excess of chloride of copper.

The fact of the solution of oxalate of lime in chloride of copper, and the subsequent precipitation of oxalate of copper, seems to be analogous to what happens if phosphate of ammonia and magnesia is prepared when a large quantity of hydrochlorate of ammonia is present. This property does not belong exclusively to chloride of copper; since other salts, as chloride of calcium, hydrochlorate of ammonia and chloride of sodium, equally retard the precipitation of the oxide of copper.

Oxalate of lime, when a large quantity of some salts is present,

as chloride of sodium, chloride of calcium and hydrochlorate of ammonia, dissolves in the chloride of copper, even when the latter is added by drops. Nevertheless in this case, the vibrations occasioned by the formation and precipitation of oxalate of copper should be as much as possible avoided, when it ceases to dissolve in the excess of the salt of copper; this precipitate is, however, formed after a certain time.

When excess of oxalate of ammonia is poured into chloride of calcium, and a salt of copper is afterwards added, no solution takes place. A precipitate of oxalate of copper is obtained, and a soluble salt of lime remains in the liquor; but if this experiment be made in presence of an excess of hydrochlorate of ammonia, the oxalate of copper does not immediately precipitate, and the solution remains clear for some time.

Oxalate of lime, boiled with soluble salts of silver, lead, cadmium, zinc, nickel, cobalt, strontia and barytes, undergoes double decomposition; so that a soluble salt of lime remains in solution, and the oxalates of these metals are precipitated.

Thus oxalate of lime is decomposed by all the soluble salts of metals capable of forming insoluble oxalates and soluble salts of lime. The decomposition takes place more readily, as the equivalent of the metal which replaces the lime is higher.

The author proposes to extend this inquiry to all the oxalates, and to examine in a more general point of view the action of soluble upon insoluble salts.—*Comptes Rendus*, Novembre 12, 1849.

ON THE PRESENCE OF HIPPURIC ACID IN THE BLOOD. BY MM. F. VERDEIL AND CH. DOLFUSS.

The authors state in a note to the Academy, that they have discovered hippuric acid in the blood of the ox. The blood upon which their experiments were performed was obtained by themselves at the slaughter-house; the experiments were repeated on the blood of many oxen, and hippuric acid was always found to be present. It was perfectly isolated from the blood, and its properties carefully studied. Not having obtained enough of the substance to submit it to an elementary analysis, they satisfied themselves that this substance was similar to the hippuric acid found in the urine of herbivorous animals, by the form of the crystals, as shown by the microscope, their insolubility in cold, and their solubility in hot water, alcohol and æther: this substance fuses by heat, exhaling the characteristic odour of benzoin. The process which the authors employed to separate this substance being connected with their general method employed in the analysis of blood, they propose to describe it when their researches are completed.—*Comptes Rendus*, Decembre 24, 1849.

GEOLOGICAL PRIZES.

The Geological Society of Dublin have offered three prizes, each of the value of Five Pounds in Books, to be awarded for the three

most valuable papers in the order of merit, that shall be communicated and read to the Society prior to the 31st of December 1850, on Theoretical or Descriptive Geology, or the application thereto of any of the kindred sciences. Competition to be free to all persons, except to Members of the Council of the Society. The Society does not bind itself to the publication of any papers presented for such competition, nor to award any prize, unless papers of adequate merit be presented.

METEOROLOGICAL OBSERVATIONS FOR DEC. 1849.

Chiswick.—December 1. Very fine. 2. Heavy rain throughout. 3. Rain. 4. Hazy clouds: fine: frosty. 5. Rain with fog: overcast at night: slight frost. 6. Clear sky and low ground-fog: exceedingly fine. 7. Overcast. 8. Rain: cloudless, and very fine. 9. Frosty and foggy: fine: dense fog. 10. Foggy: uniform haze: overcast. 11. Overcast. 12. Foggy: cloudy and cold. 13. Slight drizzle: hazy. 14. Rain: drizzly. 15. Rain: clear at night. 16. Cloudy: very fine: drizzly. 17. Boisterous: fine: clear. 18. Densely overcast: rain. 19. Cloudy: fine: clear. 20. Clear and fine. 21. Slight snow-showers. 22. Frosty: densely clouded. 23. Clear and frosty: cloudless: clear and frosty. 24. Hazy: slight snow: cloudy at night. 25. Clear and fine. 26. Drizzly: densely overcast. 27. Clear. 28. Drifting snow: clear and frosty throughout: severe frost at night. 29. Cloudy: clear. 30. Clear. 31. Cloudy: fine: overcast.

Mean temperature of the month .... 37°·17
Mean temperature of Dec. 1848 .... 41 ·75
Mean temperature of Dec. for the last twenty-three years 39 ·85
Average amount of rain in December .... 1·58 inch.

Boston.—Dec. 1. Fine. 2. Rain: rain A.M. and P.M. 3. Rain: rain A.M. 4. Fine. 5. Cloudy: rain A.M. and P.M. 6. Fine. 7. Cloudy. 8. Rain: rain A.M. 9. Fine. 10. Cloudy. 11. Rain: rain A.M. and P.M. 12. Fine. 13, 14. Cloudy. 15. Cloudy: rain A.M. and P.M. 16. Fine. 17. Cloudy: rain early A.M. 18. Cloudy: rain A.M. and P.M. 19. Fine: rain A.M. and stormy. 20. Fine. 21. Cloudy: rain A.M. 22. Cloudy. 23. Fine. 24. Cloudy: rain A.M. 25. Cloudy. 26. Fine. 27, 28. Fine: stormy. 29. Fine: snow A.M. 30. Fine. 31. Cloudy.

Applegarth Manse, Dumfries-shire.—Dec. 1. Frost A.M.: fog: wet P.M. 2. Storm of wind and rain. 3. Blowing hard: wet: calm P.M. 4. Fine clear frosty day. 5. Snow A.M: wet P.M. 6. Rain and wind. 7. High wind: rain P.M. 8. Rain, but not heavy. 9. Fog and light rain. 10. Fog all day. 11. Fair and frosty. 12. Clear and cold. 13. Dull and cold: sleet P.M. 14. Fine A.M.: cloudy and stormy P.M. 15. Foggy, with showers. 16. Fine A.M.: dull and damp P.M. 17. Fine, with slight showers. 18. Wet all day. 19, 20. Slight frost: fine. 21. Slight frost: fine: cloudy. 22. Slight frost: clear and fine. 23. Hard frost: cloudy P.M. 24. Change: soft: slight shower. 25. Frost again: mild P.M. 26. Slight frost: shower P.M. 27. Frost: clear: high wind P.M. 28, 29. Very hard frost: sprinkling of snow. 30. Frost: clear and fine. 31. Frost very hard: thermometer 18°.

Mean temperature of the month .... 37°·1
Mean temperature of Dec. 1848 .... 39 ·8
Mean temperature of Dec. for the last twenty-five years ... 38 ·1
Mean rain in December .... 1·40 inch.
Ditto average for twenty years in December .... 2·94 inches.

Sandwick Manse, Orkney.—Dec. 1. Fine: cloudy. 2, 3. Showers. 4. Snow-showers. 5. Showers. 6. Cloudy: clear. 7. Cloudy: drizzle. 8. Showers: drizzle. 9. Cloudy: showers: rain. 10. Drizzle: clear: aurora. 11, 12. Cloudy: clear: aurora. 13. Cloudy. 14. Rain: cloudy. 15. Cloudy: showers. 16. Rain: showers: clear. 17. Cloudy: showers. 18. Showers: damp: showers. 19. Bright: showers: sleet. 20. Fine: frost: fine. 21. Fine: frost: aurora. 22. Clear: frost: hazy. 23. Rain: cloudy. 24. Fine: damp. 25, 26. Showers. 27. Hail-showers. 28. Snow-drift: thunder: snow-drift. 29. Cloudy: clear. 30. Showers: cloudy. 31. Clear: cloudy.

Meteorological Observations made by Mr. Thompson at the Garden of the Horticultural Society at CHISWICK, near London; by Mr. Veall, at BOSTON; by the Rev. W. Dunbar, at Applegarth Manse, DUMFRIES-SHIRE; and by the Rev. C. Clouston, at Sandwick Manse, ORKNEY.

Days of Month.	Barometer.							Thermometer.								Wind.				Rain.			
	Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.
1849. Dec.	Max.	Min.		9 a.m.	9 p.m.	9¼ a.m.	8½ p.m.	Max.	Min.		Max.	Min.	9¼ a.m.	8½ p.m.								
1.	30·116	29·934	29·76	29·82	29·70	29·82	29·75	48	29	32	41½	27	39	44½	w.	w.	se.	s.	·12	......	......	......
2.	29·821	29·419	29·40	29·52	29·28	29·73	29·70	50	40	44	46	41	45	44	s.	sse.	se.	se.	·46	·30	......	·07
3.	29·521	29·415	29·26	29·54	29·62	29·93	30·02	40	31	39·5	40	38	41	37½	ne.	ene.	se.	se.	·06	·76	......	·07
4.	29·542	29·520	29·26	29·52	29·30	29·86	29·68	39	19	30	35	27	35	38	s.	nw.	e.	e.	......	......	......	·15
5.	29·379	29·275	29·18	29·22	29·08	29·47	29·48	47	29	30·5	39	29	41	40	s.	ese.	se.	e.	·10	......	......	·30
☾ 6.	29·722	29·640	29·33	29·20	29·50	29·38	29·57	52	34	37·5	43	36	46½	45	s.	s.	e.	s.	......	·18	......	·44
7.	29·623	29·372	29·37	29·49	29·35	29·77	29·76	49	41	40	45	38	45	42½	se.	se.	se.	se.	·10	......	......	......
8.	29·622	29·392	29·10	29·81	29·45	29·75	29·83	49	25	41	43	39½	41	43	sw.	e.	ene.	se.	·01	·33	0·50	·16
9.	30·018	29·816	29·59	29·69	29·89	29·96	30·04	45	26	34	43	42	42	45	e.	ene.	ene.	s.	......	·15	......	·07
10.	30·139	30·155	29·85	29·50	30·10	30·17	30·32	39	35	34·5	42½	39	45	39	ne.	nw.	ne.	ese.	......	......	......	·61
11.	30·129	30·089	29·74	30·12	30·11	30·36	30·36	41	34	35·5	41½	33½	39	39	ne.	ne.	e.	ese.	......	......	......	·02
12.	30·030	29·879	29·80	30·10	29·80	30·27	30·18	36	30	35	39	34	40	39	e.	ese.	ne.	ese.	·02	·10	......	......
13.	29·851	29·843	29·55	29·70	29·50	29·93	29·55	47	30	32	39	33	40	42	e.	sse.	e.	se.	......	......	......	......
● 14.	29·865	29·808	29·50	29·50	29·29	29·38	29·40	55	48	41	46	30	42	43½	s.	s.	ese.	sse.	·09	......	......	·24
15.	29·951	29·773	29·45	29·60	29·65	29·50	29·46	57	42	47	49	43	46½	47	sw.	ssw.	sw.	w.	·09	......	......	·17
16.	29·911	29·661	29·40	29·50	29·30	29·38	29·34	58	46	50	49½	44	45	43½	sw.	ssw.	sw.	w.	......	·38	0·30	·25
17.	29·859	29·525	29·14	29·28	29·50	29·31	29·42	53	40	43·5	49	42½	45	45	w.	w.	sw.	w.	......	·44	......	·07
18.	29·766	29·426	29·25	29·30	29·40	29·41	29·55	52	42	44·5	49	42	42	44	sw.	s.	sw.	nw.	·11	·06	......	·23
19.	30·236	29·878	29·50	29·90	30·10	29·41	30·15	45	32	42	47	36½	41½	38	nw.	n.	s.	nw.	·03	·26	......	·21
20.	30·312	30·233	29·92	30·25	30·36	30·38	30·51	41	30	33	48	32½	38½	38	nw.	nnw.	nnw.	......	......	·04	......	·05
21.	30·491	30·394	30·16	30·50	30·59	30·62	30·70	35	29	34·5	47	29	37½	34	ne.	ne.	sw.	s.	·02	·10	......	......
☽ 22.	30·530	30·519	30·30	30·60	30·66	30·73	30·73	37	28	32	35	31	33½	36	ne.	nne.	ne.	sw.	......	......	......	......
23.	30·572	30·516	30·33	30·62	30·46	30·50	30·55	35	22	33	33	28	46½	45½	ne.	ne.	ne.	nnw.	......	......	0·60	·31
24.	30·404	30·372	30·07	30·40	30·53	30·61	30·61	36	24	34	42	32	41½	43	nw.	nw.	ne.	nw.	·02	......	......	......
25.	30·530	30·326	30·22	30·50	30·30	30·43	30·23	38	28	33	41	29	46½	44	ne.	nw.	ne.	nw.	·01	·05	......	·10
26.	30·148	29·798	29·80	30·02	29·54	29·84	29·42	42	34	37	47	37	46½	41	nw.	wnw.	w.	nw.	·02	......	......	·15
27.	29·493	29·434	29·16	29·40	29·42	29·43	29·41	41	24	35	45	32½	32	24½	n.	nw.	nw.	n.	......	......	......	·21
28.	29·552	29·340	29·08	29·40	29·48	29·41	29·47	27	16	23	33	21	24	32	n.	nnw.	nnw.	n.	......	......	......	......
○ 29.	29·641	29·548	29·22	29·54	29·69	29·76	30·00	36	26	31	39½	22½	38	37	nw.	nw.	n.	n.	·02	......	......	·28
30.	30·219	29·952	29·80	30·05	30·20	30·18	30·20	38	28	33	38	29	37	42	ne.	nw.	n.	w.	......	·02	0·00	·06
31.	30·322	30·302	30·03	30·24	30·21	30·23	30·15	38	17	31	30	18	45	45	nw.	nw.	nnw.	w.	......	......	......	......
Mean.	29·978	29·824	29·59	29·800	29·806	29·900	29·920	43·42	30·93	36·2	42·1	33·4	40·90	40·69					1·28	3·17	1·40	4·22

THE

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[THIRD SERIES.]

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XXII. *On the connexion of Atmospheric Electricity with the Condensation of Vapour.* *By* WILLIAM RADCLIFF BIRT*.

THE intimate connexion which appears to subsist between the quantity of water diffused or suspended in the atmosphere, and the electric tension as exhibited by an atmospherical conductor, renders it not only interesting but important to trace either theoretically, or by means of experiment, the process of the formation of cloud and its resolution into rain, and the development of electricity as connected with each of these processes.

It is not my intention in the present paper to detail any new experiments, nor to bring before the reader the results of any recent investigations on this interesting subject, further than as they may incidentally throw light on any remarks which it may be found necessary to indulge in, in prosecuting the object now proposed. I shall consequently place before the reader, so far as my knowledge extends, what has been already written on the subject; and I must therefore claim his indulgence, especially in a work like this, for treading beaten ground, and presenting nothing novel to his notice. The office I propose to myself is that of a lens, which, concentrating the scattered rays of light, renders them more powerfully efficient in illuminating in some cases obscure objects.

The first writer I shall quote is John Read of Knightsbridge. In his work on atmospheric electricity published in 1793, at page 22 he has this remark relative to the relation of air to electricity: "I have no idea, much less suppose, that any, even the least quantity of electricity does ever come from air itself; for there is reason to think, that air as air, independent of the other substances, is unable to furnish any particle of

* Communicated by the Author.

this fluid; and I conjecture that pure air and electricity are actually incapable of uniting; and therefore it is that air is never known to attract, absorb, nor conduct electricity, but in proportion to the conducting substances with which it happens to be occasionally mixed: nor is air excitable of electricity by any means that is yet known; therefore air does not appear to be either an electric or a conducting body.

"It is in consequence of these peculiar properties of air that it is found to insulate the electric fluid more perfectly than any other substance."

In the above extract we are informed of the perfect *indifference* of air to electricity, whether we regard the latter as a fluid, or adopt the more modern view of its being a *force* brought into play by heat, friction, &c. Keeping steadily in mind this perfect indifference, the following extract presents us with a mode by which the electricity, or regarding it at present as a force, "the electric force," is conveyed from one portion of the atmosphere to another:—

"The electricity rises invisibly from the earth concealed in the aqueous vapour; and as the vapour ascends higher and higher into cooler air, it becomes more condensed, by which the electricity which it contains will become more condensed also*, insomuch that it will now display its energy; and in whatever direction it moves, it is sure to move actively, animated with a penetrating and expansive force; for in this state proper instruments will easily accumulate and detain it, so as to render it visible."

In this paragraph Read says "the electricity rises *invisibly* from the earth concealed in the aqueous vapour;" in other words, each particle of vapour as it rises from the earth is electrified (from what source we do not now stop to inquire), or is capable, when placed in a proper situation, of exhibiting that force which we are in the habit of terming "electric." It may be well here to contemplate each particle of vapour as surrounded by pure air, through which, as air, the electric force will not pass, *i. e.* the force cannot be communicated through it unless it is of sufficient tension to "*strike*" through the space to another conducting body. Now if by any means heat is abstracted from the vapour particles, or they pass into a stratum of cold air, they become condensed, *i. e.* they are reduced in size, contemplating each individual drop, or several run together and produce visible vapour; in both cases the

* It is well known that if equal quantities of electricity be imparted to two unequal insulated conductors, the electricity on the least surface will become more intense than on the largest. Electrified vapour is also subject to this law. [J. Read.]

surface over which the electric force was originally spread is reduced, so that it is by condensation confined to a smaller surface, and consequently ready to exert itself with greater energy, in other words, its tension is increased. If under these circumstances a substance capable of receiving and conducting the force is brought into contact with the vapour, the latter parts with its charge, or so much of it as is necessary to maintain an equilibrium between it and such bodies as we have just alluded to.

We have here presented to our notice two important particulars connected with the ascent of electrified vapour in the process of evaporation—its insulation by the surrounding air, and the increase of electric tension upon its condensation. In connexion with the insulation of the electrified vapour, or as Howard more correctly terms it, "suspended *water* (not *vapour*, for that is a *gas* and therefore a non-conductor)," Howard, in his Climate of London, vol. i. p. 148, has this important passage:—

"We can scarcely imagine a body more perfectly INSULATED *than the first particle of water*, which separating from vapour that has ascended into the higher atmosphere, begins to obey the law of gravity. There are two sources from whence such a particle may obtain an electric charge, viz. the surrounding AIR, and the vapour out of which it was formed; and which MAY (though in itself non-electric) afford to the water, *now reduced many hundred fold in volume*, A REAL POSITIVE CHARGE."

[The words printed in small capitals were scored by Mr. Howard, those in italics by myself.—W. R. B.]

It may be remarked in passing, that the views of these authors as to the *source of the electric force* differ. Read regards the force as *rising from the earth*. Howard conceives the vapour or aqueous gas to be destitute of electric force, but to exhibit it, or rather to receive a charge on its *condensation and resumption of the form of water*.

We have in the foregoing extract brought clearly before the mind a single drop of water positively electrified and perfectly insulated. Keeping this insulated electrified drop of water in view, Mr. Howard points out to us the manner in which its electric tension may be, and most probably is, increased. In his Introduction, page lxiv, in speaking of the formation of the cumulus, he says, "On these considerations we are obliged to admit as a cooperating cause of the increase of this cloud, *that sort of attraction which large insulated conducting masses exercise when charged, on the smaller ones which lie within their influence*," viz. "*to throw the small one into an*

opposite state and then attract it." This passage strikingly illustrates the increase of the electric tension by the increase of the size of the drop, *not simply by the attraction of aggregation alone*, but by the *larger* drops, considered as *insulated conductors*, throwing the *smaller* drops into an opposite state and then attracting them; so that in addition to the attraction of aggregation, by which the drops have a tendency to coalesce, the electrical attraction alluded to by Howard has a tendency to increase the size of the larger drops, or rather to produce "an agglomeration of many minute and feebly electrified globules into one rain-drop." "Thus," as Howard says in the passage which we have already quoted, "the drops of which a cumulus consists may become *larger* the *longer* it is suspended, and the electricity stronger from the *comparative diminution of surface.*" It must, however, be understood, that the larger drops thus formed are not *rain-drops* in the proper acceptation of the term, the cumulus never affording rain unless a *disturbance* of its electrical state takes place.

Under the circumstances above referred to, the cumulus and its particles of suspended water are regarded as charged conductors *bearing*, not *transmitting*, the electric force from one portion of the atmosphere to another. In this as well as the other processes that have been referred to, rain does not enter as a product. The remarks, however, of Mr. Howard on the *first formation of rain* are so extremely apposite, that in tracing the electrical action which we are capable of recognizing in the atmosphere, I shall freely avail myself of them; but as the paragraphs are rather long, I must content myself with a mere reference to the pages in which they occur.

In pages 149 and 150, vol. i. of the Climate of London, Mr. Howard speaks of the *first formation of rain* in connexion with *a double mode of the formation of cloud*; viz. the *condensation* of vapour by *refrigeration* in the *higher* regions of the atmosphere, and the production of cumuloid masses by the *condensation* in the *lower*, of the vapour immediately evaporated from the surface of the earth. In connexion with the first mode, and in accordance with the former extracts, each *particle of cloud*, each minute *drop* of water thus separated is *positively* charged; and by each (thus positively charged) obeying the law of gravitation, the whole *subside* to a region in which they remain *suspended*, forming in that region a *visible haze*. Now I presume, from what Mr. Howard has said on the formation of *cirrus*, and his comparison of it to a *transmitting* or imperfect conductor, carrying the electric force from one portion of the atmosphere to another at a great distance *over* the intervening surface of the earth (Introduction, page

lxvii and lxviii), that the resolution of this *haze* into the two diverse modifications (taking these, of course, as types of two distinct classes of clouds), *cirrus* and *cirrostratus*, depends entirely on the electric state of the circumambient air. If the *equilibrium* between the two masses of atmosphere, either superposed or on the same level and at a great distance from each other, is *disturbed*, the particles of water separated by *condensation* immediately partake (as suggested) of the nature of *conductors*, and are then drawn out into fine lines or *pencils* of condensed vapour, which transmit the electric force from one portion of the atmosphere to the other, until in some cases the equilibrium is restored. Thus cirrus is formed. On the other hand, if there is not such a disturbance of the electric state of the atmosphere as here contemplated, but the charged particles of water gradually subside so as ultimately to form, from the accumulated haze, a thin extended sheet of cloud, seen through which the heavenly bodies appear with increasing dimness, the larger luminaries being not unfrequently surrounded by well-defined halos, then the cirrostratus is formed. In the former instance, the production of cirrus, while the electric force is in process of transmission from one portion of atmosphere to another, *rain is not produced.* When the equilibrium is restored, and the fine lines of cirri *cease to transmit* the electric force, the small orbicular masses of cirrocumulus are produced; and it may be that not only each particle may retain its charge and sustain the character of an *insulated conductor*, but the congeries of particles of water forming the orbicular mass itself may possess such a character; the mass, as in the case of the cumulus, being an insulated conductor, possessing such an amount of electric tension as is necessary to preserve its independence among the other orbicular masses forming the entire cloud; each orbicular mass being considered as a component part thereof, just as each particle of water is a component part of a cumulus. According as these congeries of clouds may exist in *dry* or *moist* air, so evaporation or condensation may take place on their surfaces. In dry air they would gradually disappear; in moist air they would not only augment in size, but their electric tension might be so modified as to produce *electric attraction*, by which they might pass into the modification of cirrostratus.

In the foregoing remarks we have contemplated the passage of the condensed vapour in the higher and middle regions of the atmosphere, through the successive stages of cirrus and cirrocumulus to cirrostratus. Still keeping in mind the essential character of cirrus, and viewing its progress *without a restoration of the equilibrium*, we may inquire into the result

produced under such circumstances. In either case, whether it be a direct formation of cirrostratus from haze, or a secondary formation of this cloud from cirrus, the more perfect insulation of the charged aqueous particles which obtains in the cases of cumulus and cirrocumulus is not present. In fact the cirrostratus is a cloud more or less prevalent in a *moist* state of the atmosphere; and in contrasting its formation from cirrus with the formation of cirrocumulus from cirrus, it would appear that in a drier atmosphere, when cirrus has ceased to transmit the electric force, cirrocumulus results; while in a moister atmosphere the cirrus gradually subsides into cirrostratus. This origin of cirrostratus may be traced long after the cloud is decidedly formed, by the striated appearance which it presents to the eye when the moon is seen behind it. Mr. Howard remarks, that "when the cirrostratus is prevalent, the *lower atmosphere is usually pretty much charged with dew or haze*, and therefore in a state to conduct an electric charge to the earth."

From what has preceded, we are enabled to gather, that in the case which Mr. Howard has suggested in pages 149 and 150, the general state of the atmosphere was sufficiently moist to favour the production of cirrostratus from the haze resulting from the subsidence of the particles of water in the higher regions of the atmosphere. We may now pass on to the electrical effects consequent on the production of this sheet of haze, or its resulting sheet of cirrostratus. Mr. Howard regards the effect as the production of a negative charge in a lower stratum of the atmosphere; he says, "As soon as a sufficiently dense STRATUM of these particles is formed, we have the SUPERINDUCING CAUSE at the region *m*, *m*, *m*, by which the *lower air* may be rendered NEGATIVE; and the accumulation of such a haze before rain is not a matter of supposition only, but of long observation." In immediate connexion with this remark of Howard, it may not be inappropriate to mention, that during my late discussion of the electrical observations at Kew, I found that in most of the instances in which *negative* electricity was observed at Kew, *cirrostratus* was observed at Greenwich.

We now turn to the consideration of the production of the cumuloid masses by the condensation in the lower atmosphere of the aqueous vapour emitted by the heated earth. In connexion with this, Mr. Howard appears to regard three strata of the atmosphere as contemporaneously existing:—1st. The region of cirrostratus of course containing much aqueous vapour in a condensed state, this region possessing (or rather the cloud suspended in it) a *positive* charge; 2nd, the region

immediately below it electrified *negatively*, the depth of this stratum depending upon the thickness or density of the bed of haze above; and 3rd, the lowest stratum possessing the usual positive charge of the atmosphere. It is in this lower stratum that Mr. Howard considers the cumuloid masses to be formed; and while the negative stratum remains suspended between the superior and inferior positively electrified strata, the only result is a simple union of the cumulus below with the cirrostratus above, forming the compound modification *cumulostratus*. During this process *no rain falls*; the reason Mr. Howard appears to regard as the *absence of a transmission of the electric force*; for he says, "The effect of the superinducing charge on such cloud may produce either the CUMULOSTRATUS, which appears to be a SIMPLE UNION of clouds, or the NIMBUS, which is A UNION WITH TRANSMISSION of electricity." And it further appears from Mr. Howard's remarks, that while a stratum positively charged is interposed between the earth and the negative stratum, *rain is not produced*; but the moment the negative stratum reaches the earth, and the cumuloid masses are enveloped and lose their positive charge in it, "they then attract and are attracted by the positive haze (query cirrostratus) above, *and the first drops of rain are* [*thus*] *formed*," the cloud produced being a nimbus. "This rain opens an immediate communication with the earth; the positive electricity, which before rendered the particles buoyant, STREAMS DOWN ALONG WITH THE RAIN AND THROUGH IT; and the shower is propagated in all directions till the whole mass of cloud is brought into action." It is worthy of remark, that generally before a shower an atmospherical conductor indicates the presence of negative electricity.

Mr. Howard appears to view the mode in which *rain conducts electricity to the earth* under two aspects:—1st. "The individual drops may receive an *intense* charge at the moment of their formation and during their fall *through* the cloud, which charge they bring to the ground." Throughout the whole of Mr. Howard's reasoning, the great increase of tension *consequent on the increase in the size of the drops from the agglomeration of the minute particles of water separated by condensation* is very prominent. Under ordinary circumstances the intense charge of each drop is brought to the ground; but if the quantity of electricity is increased in a greater proportion than the surface of each drop over which it is spread, then in the very act of descending to the earth the tension may be so far increased that the electric force may separate from each drop, to seek either the surface of the cloud or of the newly-formed descending body of rain, and

an electric discharge may take place. 2nd. Mr. Howard also regards the descending body of rain as a conducting medium: he says, "the whole aggregate of floating, uniting, and falling drops from the very summit of the cloud to the ground, may form one immense conductor." He further remarks: "On the supposition that a sudden local shower is AN ATMOSPHERICAL CONDUCTOR WITH ITS FOOT ON THE EARTH, we are able to assign a satisfactory origin and use to the spreading crown, which is frequently seen above it, and in which we may discern an arrangement, tending from every side towards the dense part where the rain is formed, in a manner not required by the simple law of gravity. These rectilinear or hairy portions are the *collecting* points of the conductor formed in the *positive* haze in consequence of the destruction of the equilibrium [of its charge], which necessarily gives rise to a flow of the electricity *towards* the conductor."

Upon reviewing the successive steps in the development of cloud and rain, from the first formation of the minute water particle with its *feeble electric charge*, to the torrent-pouring nimbus with its *violent electric discharge*, we have particularly to trace the formation of this cloud in a portion of atmosphere bounded above by a sheet of cirrostratus *positively* electrified, which rests on a stratum of air *negatively* electrified, this negative state being *induced* by the action of the positive cirrostratus, which at this time is *highly charged* and ready to precipitate, the clouds below being negatively electrified by induction. The atmospheric conductors are also at this time negatively electrified, in common with other bodies on or near the earth. This (according to Howard's remarks) appears to be the moment when the nimbus is formed; the disturbance, which had been produced *very gradually*, now makes itself distinctly felt; the masses of cumulus, originally positive, by the superior energy of the cirrostratus, are more or less *thrown into a negative state*; electrical attraction between the cirrostratus and cumulus rapidly takes place, and is accompanied by the agglomeration of the minute particles of water forming the clouds; rain immediately descends, which, if the electric tension either of the cloud or of the descending rain-drops is not too intense, becomes a conducting medium, and opens an electrical communication with the earth; but if the charge of the cloud, on the other hand, becomes so high as to result in a violent detonation, either before or coincident with the formation of rain, or the rain-drops themselves are incapable of retaining the charge received by them at the time of their formation, then we have with the nimbus all the phænomena of a thunder-shower.

Mr. Howard, in his remarks on the formation of nimbus (Introduction, page lxxi), a cloud very closely allied to the cumulostratus, expresses himself in almost the precise words of the Committee of the Royal Society*. "The cirri, also, which so frequently stretch from the superior sheet upwards and resemble erected hairs, carry so much the appearance of temporary conductors of *the electricity extricated by the sudden union of its minute drops into the vastly larger ones that form the rain*, that one is in a manner compelled, when viewing the phænomenon, to indulge a little in electrical speculation." The sudden agglomeration alluded to by the Committee is here unmistakeable; and it would appear from the course of reasoning developed in the preceding extracts, that the greater manifestation of the *electrical attraction*—noticed under the head of "formation of cumulus"—consequent upon the coalescence or union of the cumulus with the cirrostratus, which is generally crowned with cirrus, is most probably—especially when combined with the pressure, in the lower part of the cloud, of the rising; and the deposition on its upper surface of the descending vapour—the occasion of the disturbance of the electrical state of the cumulus by which the rain is produced. We have, in fact, the entire process beautifully presented to our notice, commencing with the incipient separation of the earliest drops of water by the agency of a diminished temperature from the vapour *rising above the vapour plane*, which drops are feebly electrified and more or less perfectly insulated; the process terminating with the sudden agglomeration of these minute and feebly electrified drops into the vastly larger ones that form rain, by which the electric tension may be, and doubtless is, so enormously increased, that the electric state of the cloud and of bodies in its immediate neighbourhood are extensively disturbed; so that if the superabundant electricity should not be carried off by the *conducting* or rather *transmitting* crown of cirri, as suggested by Howard, the exhibition of electrical phænomena, as thunder and lightning, the stroke being given either by the *cloud* or the *rain*, generally follows. The intermediate steps appear to be governed by the "two grand predisposing causes—a falling temperature and the influx of vapour." Should these not be sufficient in their extent, either above or below, to produce a *formation of cirrus* or an increase of cumulus, the individuals of the latter modification gradually evaporate as evening approaches, and generally give place to a serene and tranquil night; but if both these classes of cloud form rapidly, cirrostratus generally shows itself, electrical action is called more

* Philosophical Magazine, vol. xxxv. p. 161, foot note.

energetically into play, and upon the formation of cumulostratus and nimbus, the rain is produced in the manner above indicated.

Throughout the whole of the preceding remarks there is one point presented to our notice with peculiar prominence, viz. the development of the electric force in the act, more or less, of the condensation of vapour. The entire process of nubification appears to be intimately connected with this development, and the different modifications of clouds entirely dependent on the presence or absence of a disturbed electrical state, not only when the condensation is effected, but when no previous disturbed state exists it may actually be brought about by the condensation of vapour. It may not be too much to say, that the electricity observed in the atmosphere is principally due to, and the product of condensed vapour. We would of course not regard the condensation of vapour (if the preceding conjectures be correct) as the only source of atmospheric electricity; but we are inclined to regard it as by far the most productive; and it now only remains to connect the foregoing conjectures and reasoning with such experiments as may be calculated to throw some light on the production of the electric force by condensation.

Mr. Reuben Phillips has lately detailed in the Philosophical Magazine some very interesting experiments on the electricity of condensation. In one experiment a jet of water was passed through a jet of steam in such a way that the electricity of the water should not affect that of the steam, the water *only* falling on the wire-gauze of the collector; a negative charge was obtained until the force became sufficiently strong to effect a change in the kind of electricity; in other words, the jet of water exhibited precisely the same effect as the steam alone. Mr. Phillips attributes this to the drops of water *collecting negative electricity from the steam*: he says, "The negative charge given to the screen was I think only produced by the drops of water *collecting negative electricity from the steam*, much in the same way as the wire-gauze did *."

We here see the drops of water regarded in the light of *insulated* (?) conductors projected into the midst of condensed vapour. Each particle of this vapour is electrified negatively; and as the particles of water either come in contact with or sufficiently near to receive the charge of the particles of vapour, the charge passes to the water-drops, and through the medium of the screen affects the electrometer. It would be very interesting to know, if with a given pressure of steam, such, for instance, as would produce the *greatest* negative

* Page 106 of the present volume of this Journal.

tension, the passage of a jet of water properly guarded so as to give no electrical indications would *increase* the tension; in other words, would the condensation or running together of the vapour particles cause an increase of tension in the way suggested by the Committee of Physics of the Royal Society?

Mr. Phillips's experiment, that bears more particularly on the point in question, is the mixture of steam and water in a tin pipe. When the steam was introduced alone, no electrical effects were exhibited; and when the water was introduced alone, there were also no electrical effects; but when they were mixed, a positive charge was indicated by the electrometer. The experiment was thus varied: on one occasion the steam was first introduced; and upon the jet of water being projected among the particles of steam, the mixture was electrified positively: on the other occasion the water was first introduced; and upon the jet of steam being projected into the tin pipe, the mixture was also electrified positively. These two experiments appear to me to be conclusive on the subject of the development of the electric force by condensation *alone*, and become a link in the chain of evidence by which nearly all the varied, beautiful, grand and terrific phænomena of the atmosphere are bound together. This one simple but efficient mode of the development of the electric force appears to be fully capable of accounting, if not for the whole, for the greater portion of the phænomena of atmospheric electricity.

Kew Observatory, Feb. 6, 1850.

XXIII. *Some Observations on a New Equation in Hydrodynamics. By* Professor P. Tardy *of Messina**.

I HAVE lately seen an article by Professor Challis in the Number of the Philosophical Magazine for last June (Supplement), "On certain points relating to the Theory of Fluid Motion," in which, in reply to a communication of M. Bertrand to the French Academy of Sciences, he returns to his favourite argument for the truth and necessity of a new equation in hydrodynamics. At the same time he does me the honour of mentioning my name, and answering an objection which I made to his analysis for the case of the motion in space of two dimensions.

I must premise that when I wrote my memoir on the movement of fluids (*Sopra alcuni Punti della Teoria del Mòto de' Liquidi*, Firenze 1847), I had no knowledge of Professor Challis's

* Communicated by the Author.

original papers inserted in the Cambridge Philosophical Transactions and in the Philosophical Magazine, and was only aware of a brief extract of some of his results given in Webster's Theory of Fluids. I have subsequently endeavoured to peruse the many hydrodynamical memoirs of Professor Challis, and I am obliged to confess that I have found nothing in them that could change my first opinion, or make me dissent from M. Bertrand's assertions. I wish for the moment merely to offer some remarks on the new equation alleged to be necessary.

The learned Professor having established by elementary considerations the equation

$$\frac{d\rho}{dt}+\frac{d.\rho\mathrm{V}}{ds}+\rho\mathrm{V}\left(\frac{1}{r}+\frac{1}{r^1}\right)=0, \quad . \quad . \quad . \quad (1.)$$

where ρ is the density of the fluid, V the velocity at the point x, y, z, ds the differential of the arc of the line of motion, and r and r' the principal radii of curvature at the same point of the surface normal to the directions of motion, has repeatedly asserted that this equation could not be derived from the two known equations

$$\frac{d\rho}{dt}+\frac{d.\rho u}{dx}+\frac{d.\rho v}{dy}+\frac{d.\rho w}{dz}=0, \quad . \quad . \quad . \quad . \quad . \quad (2.)$$

$$\left.\begin{aligned}(dp)=\rho\Big\{\Big(\mathrm{X}-\Big(\frac{du}{dt}\Big)\Big)dx+\Big(\mathrm{Y}-\Big(\frac{dv}{dt}\Big)\Big)dy\\ +\Big(\mathrm{Z}-\Big(\frac{dw}{dt}\Big)\Big)dz\Big\},\end{aligned}\right\} \quad (3.)$$

(according to the notation employed by him), and that therefore another general equation was requisite, from the combination of which with equation (2.) equation (1.) may result. This is the *only* argument I have been able to find in the writings of Professor Challis for the necessity of a third equation, and he even confesses that he knows no other use for it but that of deducing equation (1.). (Phil. Mag., vol. xxxiii. No. 223.)

Now to investigate this, he assumes that there always will be a factor $\frac{1}{\lambda}$ capable of rendering integrable the expression $udx+vdy+wdz$, so that

$$\frac{u}{\lambda}dx+\frac{v}{\lambda}dy+\frac{w}{\lambda}dz=(d\psi). \quad . \quad . \quad . \quad (4.)$$

The integral of this,

$$\psi(x,y,z,1)=0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5.)$$

where an arbitrary function of the time is included in ψ, re-

presents, as is known, a surface cutting at right angles the directions of motion, which we will also call a *surface of displacement.*

From this Professor Challis deduces

$$\frac{d\psi}{dt}+\frac{d\psi}{dx}u+\frac{d\psi}{dy}v+\frac{d\psi}{dz}w=0 \quad . \quad . \quad . \quad . \quad (6.)$$

by reasoning which I, like the illustrious analyst M. Bertrand, am unable to appreciate. Hence is evident the passage from (6.) to

$$\frac{d\psi}{dt}+\lambda\left(\left(\frac{d\psi}{dx}\right)^2+\left(\frac{d\psi}{dy}\right)^2+\left(\frac{d\psi}{dz}\right)^2\right)=0, \quad . \quad . \quad . \quad (7.)$$

which is the new equation he contends for.

In his reply, Professor Challis, contradicting his former assertions, states, without further explanation, that the use of equation (7.), which he had already employed, *is not absolutely necessary* for arriving at the equation (1.). Thus falls to the ground his chief argument for the necessity of a new general equation. And in fact it is easy to convince oneself that the equation (1.) is only an analytical transformation of (2.). From equation (4.) we have

$$u=\lambda\frac{d\psi}{dx}, \quad v=\lambda\frac{d\psi}{dy}, \quad w=\lambda\frac{d\psi}{dz};$$

differentiating the first of these with respect to x, we obtain

$$\frac{du}{dx}=\lambda\frac{d^2\psi}{dx^2}+\frac{d\lambda}{dx}\frac{d\psi}{dx}.$$

Putting for the sake of brevity

$$\left(\frac{d\psi}{dx}\right)^2+\left(\frac{d\psi}{dy}\right)^2+\left(\frac{d\psi}{dz}\right)^2=\mathrm{R}^2,$$

and observing that

$$\frac{\frac{d\psi}{dx}}{\mathrm{R}}=\frac{u}{\mathrm{V}}, \quad \lambda=\frac{\mathrm{V}}{\mathrm{R}},$$

and thence

$$\frac{d\lambda}{dx}=\frac{\frac{d\mathrm{V}}{dx}}{\mathrm{R}}-\mathrm{V}.\frac{\frac{d\psi}{dx}\frac{d^2\psi}{dx^2}+\frac{d\psi}{dy}\frac{d^2\psi}{dxdy}+\frac{d\psi}{dz}\frac{d^2\psi}{dxdz}}{\mathrm{R}^3},$$

there results

$$\frac{du}{dx}=\frac{u}{\mathrm{V}}\frac{d\mathrm{V}}{dx}+\frac{\mathrm{V}}{\mathrm{R}^3}$$

$$\left\{\mathrm{R}^2\frac{d^2\psi}{dx^2}-\left(\frac{d\psi}{dx}\right)^2\frac{d^2\psi}{dx^2}-\frac{d\psi}{dx}\frac{d\psi}{dy}\frac{d^2\psi}{dxdy}-\frac{d\psi}{dx}\frac{d\psi}{dz}\frac{d^2\psi}{dxdz}\right\}.$$

In like manner we obtain the values of $\frac{dv}{dy}$ and $\frac{dw}{dz}$; and by adding and having regard to the known expression of $\frac{1}{r}+\frac{1}{r^1}$, and to the identity,

$$\frac{dV}{ds}=\frac{dV}{dx}\cdot\frac{dx}{ds}+\frac{dV}{dy}\cdot\frac{dy}{ds}+\frac{dV}{dz}\cdot\frac{dz}{ds}=\frac{dV}{dx}\cdot\frac{u}{v}+\frac{dV}{dy}\cdot\frac{v}{V}+\frac{dV}{dz}\cdot\frac{w}{V},$$

there results

$$\frac{du}{dx}+\frac{dv}{dy}+\frac{dw}{dz}=\frac{dV}{ds}+V\left(\frac{1}{r}+\frac{1}{r^1}\right);$$

after which the passage from (2.) to (1.) presents no difficulty. Having thus shown that the equation (6.) or (7.) is not *necessary*, it remains to be examined whether it is *true*.

Nor can Professor Challis say that, having employed the equation (7.) in combination with that of continuity, and having thus arrived at a true consequence, viz. at the equation (1.), the truth of (7.) is in this manner established; because on examining his calculation, it is easy to perceive that a compensation of errors has taken place; and, indeed, as λ is a quantity that disappears from the final result, it was indifferent to have substituted a false value of it. This will more distinctly appear if we take the expression of $\frac{1}{r}+\frac{1}{r^1}$, and put for $\frac{d\psi}{dx}$, &c. $\frac{u}{\lambda}$, &c. ..., because all the terms which contain λ and its differential coefficients evidently destroy each other.

I shall begin by observing that Professor Challis ought to have shown that the equation

$$u\left(\frac{dv}{dz}-\frac{dw}{dy}\right)+v\left(\frac{dw}{dx}-\frac{du}{dz}\right)+w\left(\frac{du}{dy}-\frac{dv}{dx}\right)=0$$

is always satisfied in order that $udx+vdy+wdz$ should become integrable by a factor. Moreover in his last communication he explains what the equation (6.) means; but he does not assign any new and valid reason for the passage from (5.) to (6.). We were already well-acquainted with that signification; but we desired to know how he could show that the particles of the fluid which are on the surface $\psi=0$ must remain on it during successive instants. Nay, if the motion is steady, and we add only an absolute constant in the integration of (4.), and therefore take $\frac{d\psi}{dt}=0$, is it not evidently absurd to affirm that the particles of the fluid move along the surface to which the directions of their motion were normal? In general it is known that, in order that the particles of the fluid may always remain on a surface $\psi(x, y, z, t)=0$, it is neces-

sary that the time should disappear from this equation whenever for the coordinates x, y, z are substituted their expressions formed with the initial values a, b, c, and the time t, as Lagrange first noticed. Now as ψ is supposed to contain an arbitrary function of t, it is clear that this cannot be the case. Besides, if the equation (7.) be exact, as we can augment the partial differential coefficient $\frac{d\psi}{dt}$ by an arbitrary function of the time $\chi(t)$, it would follow that

$$\frac{R^2}{\frac{d\psi}{dt}} \text{ and } \frac{R^2}{\frac{d\psi}{dt}+\chi(t)} = \frac{\frac{d\psi}{dt}}{\frac{d\psi}{dt}+\chi(t)} \cdot \frac{R^2}{\frac{d\psi}{dt}}$$

are at the same time factors which render integrable $udx+vdy+wdz$. Hence it ought to be

$$u\frac{d}{dy}\cdot\frac{\frac{d\psi}{dt}}{\frac{d\psi}{dt}+\chi(t)} = v\frac{d}{dx}\cdot\frac{\frac{d\psi}{dt}}{\frac{d\psi}{dt}+\chi(t)}, \text{ \&c.,}$$

or

$$\frac{u}{v} = \frac{d\psi}{dx} : \frac{d\psi}{dy} = \frac{d^2\psi}{dxdt} : \frac{d^2\psi}{dydt}, \text{ \&c.,}$$

which would lead to a peculiar form of ψ.

I subjoin an example which, I think, evidently shows the inconsistency of the equation (6.). Let us suppose the fluid to be homogeneous and incompressible, and of a density $\rho=1$, and let us take

$$u=y(t-1), \quad v=x(t+1), \quad w=0.$$

The two equations (2.) and (3.) are both satisfied, and the latter gives

$$p=f(t)+W-yx-(t^2-1)\frac{x^2}{2}+(t^2-1)\frac{y^2}{2},$$

where $f(t)$ is an arbitrary function of the time, and

$$W=\int(Xdx+Ydy+Zdz).$$

Now $udx+vdy$ is not an exact differential, and the factor capable of rendering it so is $\frac{1}{\lambda}=\frac{1}{xy}$. Then the equation (5.) becomes

$$\psi=x^{t-1}.y^{t+1}+\chi(t)=0,$$

$\chi(t)$ being an arbitrary function of t; and the equation (6.)

would give

$$\log xy+(t-1)^2\frac{y}{x}+(t+1)^2\frac{x}{y}-\frac{1}{\chi(t)}\cdot\frac{d\chi(t)}{dt}=0,$$

a result which we would beg Professor Challis to justify.

I have anxiously searched in the memoirs of Professor Challis for an instance of applying the new equation, and I must confess that I remained quite astonished when, in the Cambridge Philosophical Transactions, vol. viii. part 1, I found the following example. If

$$u=mx, \quad v=-my, \quad w=0,$$

we have

$$udx+vdy=m(xdx-ydy). \quad . \quad . \quad . \quad . \quad (8.)$$

Professor Challis takes for the integral of this, or for the general equation of the surfaces of displacement,

$$\psi=x^2-y^2-a^2=0. \quad . \quad . \quad . \quad . \quad . \quad . \quad (9.)$$

Hence from equation (7.)

$$\lambda=\frac{a\frac{da}{dt}}{2(x^2+y^2)}.$$

But, adds Professor Challis, by the equation (8.) we have

$$x\frac{dx}{dt}-y\frac{dy}{dt}-a\frac{da}{dt}=0;$$

and since

$$\frac{dx}{dt}=u=mx, \quad \frac{dy}{dt}=v=-my,$$

$$\therefore a\frac{da}{dt}=m(x^2+y^2);$$

therefore, substituting $\lambda=\frac{m}{2}$, which value, he says, *makes* $\frac{u}{\lambda}dx+\frac{v}{\lambda}dy$ *an exact differential, and the equation* (7.) *is therefore verified.* I can scarcely conceive how such an illogical process has escaped the sagacity of the learned Professor. And indeed what does Professor Challis do but take the value of λ from the equation (7.),

$$\lambda=-\frac{\frac{d\psi}{dt}}{\left(\frac{d\psi}{dx}\right)^2+\left(\frac{d\psi}{dy}\right)^2+\left(\frac{d\psi}{dz}\right)^2},$$

and then substitute in it for $\frac{d\psi}{dt}$ the value deduced from (6.),

$$\frac{d\psi}{dt} = -\left(\frac{d\psi}{dx}u + \frac{d\psi}{dy}v + \frac{d\psi}{dz}w\right)?$$

and what can be the result of this but the identity $\lambda = \lambda$?

If he has obtained $\lambda = \frac{m}{2}$, it is because in taking the equation (9.) for the integral of (8.), he has multiplied the last by $\frac{1}{\lambda} = \frac{2}{m}$. Nor has the following deduction in the last communication of Professor Challis caused me less surprise.

He supposes the fluid to be incompressible, in which case the equation (1.) becomes

$$\frac{dV}{ds} + V\left(\frac{1}{r} + \frac{1}{r^1}\right) = 0;$$

and says, "since $ds = dr = dr^1$" we have by integrating

$$V = \frac{\varphi(t)}{rr^1}; \quad \ldots \ldots \quad (10.)$$

and if the motion take place in space of two dimensions,

$$V = \frac{\varphi(t)}{r}. \quad \ldots \ldots \quad (11.)$$

The assumption of $ds = dr = dr^1$ is so strange, that it is needless to spend many words in order to demonstrate the incorrectness of the result. I shall only observe, that the value of V may be put under the form

$$V = \frac{A}{rr^1 d\tau d\tau^1},$$

where $d\tau$, $d\tau^1$ are the angles of contingence of the two lines of curvature at the point x, y, z of the surface of displacement, and A is variable, not only with the time, but also from one line of motion to another. The equation (11.), says Professor Challis, proves the proposition to which I had made some objections; but my former remarks were limited to the assumption of equal forms for the two arbitrary functions in the integral of

$$\frac{d^2\varphi}{dx^2} + \frac{d^2\varphi}{dy^2} = 0,$$

and, in consequence, to the conclusion, that in the hypothesis

of $udx+vdy=d\phi$ the movement is always rectilinear. The equation (11.), even were it true, proves nothing; it remains to demonstrate that, in the case above-mentioned, the lines of displacement are always circular.

Florence, January 8, 1850.

XXIV. *On the Deportment of Crystalline Bodies between the poles of a Magnet.* *By* John Tyndall *and* Hermann Knoblauch*.

THE results obtained by Professor Plücker of Bonn, in his investigations upon crystals, induced us early in the month of November last to commence a series of researches in connexion with this subject. Our inquiries, so far as they at present reach, form the subject of this paper.

After a long series of trials, not necessary to be recapitulated here, we arrived at the persuasion that no safe inference could be drawn from experiments made with full crystals. It appeared necessary to examine the forces attributed to crystalline bodies in detail, one at a time, removing as far as possible all influences likely to interfere with the simple action of this one.

To attain this object we experimented with cubes: we had one cut from tourmaline, in such a manner that the optical axis of the crystal ran parallel to four sides of the cube; on suspending it between the poles and closing the circuit, the optical axis set itself strongly equatorial; thus corroborating the law of Plücker, which affirms that the optical axes of negative crystals are repelled. When, however, the same cube was hung with the optical axis vertical, the influence of that axis being thus destroyed, a preference was shown to one of the diagonals of the horizontal face of the cube, not to be explained by the law mentioned; this preference was more strikingly exhibited in the case of the following two crystals.

A cube of beryl, cut similarly to the tourmaline, being hung with its optical axis vertical, one diagonal of its horizontal face set itself axial; only one diagonal could maintain this position, the other was repelled. In dichroite this phænomenon was very strikingly exhibited; when hung with the middle line of the optical axes vertical, one diagonal assumed the axial position; if, however, the circuit was closed when the other diagonal chanced to lie from pole to pole, the latter seemed to experience a repulsive shock, sufficient to make the cube spin several times round upon its axis.

* Communicated by the Authors.

We do not see any possibility of referring this election of a particular diagonal to the influence exerted by the optical axis, at least in the case of beryl.

To ascertain the exact nature of this influence, we had recourse to discs, cut so that the optical axis of the crystal lay as a diameter in the plane of each. In this way the influence of mere form was totally annulled, and the pure action of the optical axis, if such existed, might be observed.

Our first discs, five in number, were taken from a semi-transparent crystal of Iceland spar, and lay at various angles to the sides thereof. In all these cases the law of Plücker was strictly verified, the optical axis being always repelled.

Four discs and one square were next taken from two transparent crystals of the spar, and suspended successively between the poles. We were by no means prepared for the reply given to these experiments; *in each of the five cases the optical axis set itself distinctly axial.*

The balance sheet of our inquiries up to the present time is this: out of eleven crystals of Iceland spar examined as above, five have obeyed the law of Plücker, while six have contradicted that law.

In determining whether the optical axis will be repelled or not, it is not necessary to cut the crystals in the manner described. A thin rhomb cloven from the crystal and ground into the shape of a disc, will decide the question. If it belong to the class whose optical axis is repelled, the line bisecting the acute angles of the rhomb will set itself axial; if to the other class, the same line will set itself equatorial.

Discs thus prepared form undoubtedly the purest means of investigating this question. The rhomb itself, however, without being ground into a disc, affords us sufficient intelligence as to the class of the crystal to which it belongs. Is its optical axis repelled, then the long diagonal of the rhomb will incline to the axial position; is the optical axis attracted, the long diagonal will stand nearly equatorial.

The same adherence of the diagonal to the axial or equatorial position continues after a thin bar containing the diagonal has been severed from the rhomb. For example, a bar containing the short diagonal of that class whose optical axis is attracted, will stand nearly axial. This fact is perhaps worthy of notice: if Iceland spar be diamagnetic, as Prof. Plücker asserts, and if the optical axis be repelled, what is it that overcomes the united action of both in the case of this diagonal? The projection of the optical axis lies in the same direction as the bar; both therefore work together, and both strive, in virtue of the two properties mentioned, to attain the

equatorial position: they do not attain it, however; the bar stands almost axial.

The question, "Is the substance magnetic or diamagnetic?" necessarily lay at the threshold of all attempts to explain these phænomena. To answer this question by experimenting with the full crystal was impossible, as a diamagnetic crystal, it is well known, can set itself axially. This being evidently owing to some hidden property of the crystalline structure, we thought it might be destroyed by reducing the mass to powder. Portions of crystals of each class were finely pounded in an agate mortar; by the addition of a little distilled water the powder was made into a paste, from which small bars were constructed and carefully dried. On being hung between the excited poles, those whose optical axes were repelled stood equatorial, the others axial.

Adopting the plan already followed by Faraday, we next brought the two classes of crystals to the test of a single pole. At first, little bits of crystal were attached to the cocoon thread by means of a soft sticky kind of wax; the presence of this, however, was found to interfere with the purity of the experiment, and it was therefore abandoned; fine silver wire was next tried and also found ineligible; we next hung a straw horizontally, into each end of which a bit of crystal was thrust; but the straw was diamagnetic, and permitted no safe conclusion. Common white taper-wax was at length found exactly suited to our purpose; it must, however, be handled with clean fingers, and even thus very little; after two or three suspensions it invariably showed signs of magnetism. We chose long thin bars of crystal and hung them vertically, thus bringing the wax so far above the poles, that, on examination, it showed not the slightest trace of magnetism or diamagnetism. A former remark explains why this vertical hanging is to be preferred to horizontal; in the latter case, a rotation towards the pole, easily mistaken for an attraction, and difficult to distinguish from it, might occur with a diamagnetic body; hung vertically, however, it could be distinctly seen whether the *mass* of the crystal was attracted or repelled. The results here delivered were in harmony with those already mentioned: those whose optical axes were attracted, were attracted; those whose optical axes were repelled, were repelled.

Anxious to investigate this difference of action to the bottom, we chose two perfectly pure and transparent crystals from each class, and submitted them to chemical analysis. An experienced mineralogist was unable to detect the slightest visible difference between these crystals; the analysis, how-

ever, showed that those whose optical axes were attracted contained protoxide of iron in considerable quantity, while those whose optical axes were repelled contained no trace of this metal.

Here, then, we have two crystals perfectly alike in optical respects, but chemically different; the change in the position of the optical axis between the poles being doubtless due to this difference. This seems to reduce that position to a mere function, so to speak, of the chemical nature of the substance. Could a salt of iron be introduced as an isomorphic substitute for some other constituent in the whole class of diamagnetic crystals, it is exceedingly probable that the position of the optical axis between the poles would in most of these cases be reversed, and this without in the slightest degree interfering with the optical properties of the crystal. It is even likely that Nature, as in the case before us, furnishes many examples of this isomorphic substitution. If this be true, then the position of the optical axis between the poles is a mere accident, and the introduction of it can only serve to render this already difficult subject unnecessarily complex.

On bringing a circular disc of gutta percha, which, in its manufacture, appeared to have a fibrous structure imparted to it, between the excited poles, the direction of these fibres set itself strongly axial. This action was so decided, that a parallelogram, three-quarters of an inch long and half as wide, with the fibres crossing it transversely, set itself stiffly equatorial. This can by no means be referred to the distance of the parallelogram from the poles, or to any other of those circumstances by which diamagnetic action is said to be exhibited; our voltaic power varied from one to twenty cells of Bunsen's battery, but the result remained specifically constant; further, on being hung edgeways, the parallelogram stood strongly magnetic; and when one pole was removed, the whole mass was attracted by the other.

Whence, then, this apparent diamagnetism of the gutta percha? The answer to this question will perhaps throw a light upon the complicated phænomena exhibited by crystals generally. The equatorial position of the gutta percha is manifestly due to the comparative facility with which the magnetic force can act in the direction of the fibre. Let us suppose the parallelogram suspended, and the circuit closed; every point of its substance is now affected, but not with equal force in all directions; in the direction of the fibre the action is strongest, and may be represented by the longer diameter of an egg, in the centre of which the point may be imagined. All lines drawn from this centre to the shell will represent the

amount of the magnetic force in their various directions. On this assumption the equatorial position is readily explained; and the necessity of the parallelogram, when hung edgeways, to set itself axial, is also manifest.

As may be expected, when the parallelogram is made very long in comparison to its width, the long diameter of our hypothetical egg is overpowered by the united action of a number of short ones, and the oblong stands axial.

We have succeeded in obtaining analogous results with ivory, which, though diamagnetic, can be so cut that it stands almost axial. The anomaly is explained by reference to the structure of the tooth, which modifies, in certain directions, the diamagnetic power. By attending to these circumstances, we have been able, with these two substances, gutta percha and ivory, to imitate almost all the experiments which we have made with both classes of crystals.

If we suppose the shorter diameter of an ellipse to coincide with the straight line formed by the intersection of any two surfaces of cleavage, and the ellipse to rotate around this diameter, an oblate spheroid will be the result. Conceive lines drawn through the centre of this figure and terminated by the surface, to represent the amount of magnetic or diamagnetic force in the direction of these lines, and we have an hypothesis of magnetic or diamagnetic action within the crystal, sufficient, not only to account for every fact noticed in this paper, but for numerous others, the discussion of which we refer to a future occasion.

Extending this principle to the intersections of the three surfaces of cleavage, we obtain a resultant which falls in the direction of the principal axis of the crystal, or of the optical axis. The position of that resultant between the poles will depend solely upon the magnetism or diamagnetism of the crystal, and in nowise upon the fact of its being negative or positive, as asserted by Professor Plücker.

It is highly improbable that our representative spheroid will be of a constant shape in all crystals: in the case of gutta percha we assumed it formed by the rotation of a semi-ellipse round its longer axis, or what is commonly called prolate; in the case of Iceland spar it is oblate; in common iron it would be a sphere, as here the magnetic force appears to act equally in all directions. Every crystal will doubtless modify it in a manner peculiar to its own substance and structure. Future experiments will perhaps enable us, in many cases, to determine the numerical values of the long and short diameters of these spheroids.

From these considerations it would follow, that M. Plücker,

in attempting to refer the facts observed by Mr. Faraday* to the optical axis, inverts the right course of proceeding; the attraction or repulsion of this axis being a secondary result, depending first of all upon the magnetism or diamagnetism of the substance, and secondly upon the manner in which either force is modified by the peculiar structure of the crystal.

The conducting power, so to speak, of Iceland spar for both magnetism and diamagnetism appears to be in directions perpendicular to the lines of cleavage. If these views be correct, the optical axis can no longer be regarded as the prime agent in the production of the phænomena which we have been considering; we shall no longer seek the explanation of new facts in the hypotheses of new forces, but rather in modifications of the old.

Marburg, January 1850.

XXV. *Further Illustrations of a Method for computing Magnetic Declination, on the principle proposed by* Professor Gauss. *By* Samuel Beswick†.

In my former communication I gave two illustrations of a new method whereby the mean declinations at Greenwich and St. Helena, for the past year 1849, were computed with the utmost exactitude. But the utility of this new method consists, not only in its simplicity and precision, but also in its applicability to *all places* in the two hemispheres, and to all epochs, past, present, and to come. To obtain the declination all over the world, at an epoch so distant as the time of Columbus (see the first illustration), or of one hundred, or even one thousand years in the *future*, there needs no additional data, nor any alteration in the method: there are only the same number of items: and the result is obtained equally easy, and in the same time, as if the declination was being obtained for the *present year*. *Time* and *place* make no difference in the length or nature of the computation.

If, therefore, this paper be favoured with the attention of the Committee appointed to conduct the co-operation of the British Association in the system of Simultaneous Magnetical and Meteorological Observations, I trust they will kindly notice, not only the above observations, but also the following advantageous characteristic of this new method over all others, viz. to compute the declination all over the world, for any epoch, however remote the time may be, *not a single observa-*

* Phil. Mag., Jan. 1849, p. 75.

† Communicated by the Author.

tion of any kind is required. The two accompanying illustrations are intended as proof of its advantageous superiority in this particular. Colonel Sabine has kindly reminded me of the *grand desideratum* in this department of science, for the accomplishment of which enormous labours have been undertaken, and considerable sums of money annually expended,—that "the chief difficulty in any empirical formula would be to adapt it to different epochs." I have thought it necessary, in consequence of this remindal,—and with a view to the complete exposition of the method I propose, and to the clear understanding of the important practical advantages it offers over all others,—to present a few general explanatory remarks, accompanied with two distinct proofs of its application to different epochs. Which proofs are intended to show, that this 'chief difficulty' is completely removed; indeed so completely, that not a single item would be added to the process, nor even altered in its form, whether the epoch was *three* years in the past, or *three hundred* years in the future.

I am desirous of recording a matter of considerable importance in this paper, to which I solicit the kind attention of the gentlemen forming the Committee for Simultaneous Magnetical and Meteorological Observations referred to above. The obvious failure of all previous methods, after the interval of a few years, mainly results from a quarter which, up to the present time, is not even suspected. It is this—*the southern magnetic point* of convergence of the horizontal force, or pole, *does not revolve from east to west*, as hitherto supposed; on the contrary, *both poles move in the same direction* from west to east. Hence, if the numerical coefficients of Prof. Gauss's general theory be reconstructed—the necessity of which is suggested by Colonel Sabine*,—it can never become available for practical purposes, except for the time being, in consequence of this fundamental error. Every formula should be required to prove its utility by its application to successive epochs of 50 or 100 years interval for 300 years past, before it be relied on in its applications to the future. Should this test be admitted, then my investigations into this matter enable me to state, that the general formula of Gauss, and of all others which suppose a revolution of the southern magnetic pole from E. to W., will prove, during the test, an entire failure. I would respectfully suggest to the Committee of Observation, that every method be required to fulfill the demands of this test.

We will now try the merits of our own method by this rule; and, in so doing, we wish to be followed by all others pro-

* Phil. Trans., part 2, 1849.

posing to compute magnetic declination. The epochs selected are the years 1492 and 1722: the first is 357 years in the past, and the latter 127 years. The epoch of 1492 refers to the celebrated discovery of the magnetic line of no declination, during the first voyage of Columbus; and the epoch of 1722 refers to Graham's excellent observations on the declination of the needle at London. The epochs are as memorable as any in the history of terrestrial magnetism, and are as widely distant from each other, and from the present, as any which can be selected for the trial we propose.

In the first place, we will state the data on which we ground the adaptation of our method to different epochs. The data simply consist in discovering the annual velocity of the north and south magnetic poles; for when this is obtained, their situation at any given epoch is easily found, by multiplying the annual velocity of each by the number of years intervening between the epoch and a given year (1849), when their relative situation is known. After a long and almost hopeless investigation, I have ascertained the velocity in longitude of the north magnetic pole to be double that of the magnetic pole in the southern hemisphere, and that both have had a diminishing velocity, with occasional exceptions*, since the epoch of 1492. The rule for computing the velocity and situation of the two poles is as follows:—Commencing with the year 1460, multiply the whole interval between that year and 1849 (389 years) by seventeen minutes; and for every successive diminution of 60 years in that interval, lessen the multiplying number one minute, as in the following table. The fixed epoch by which the interval is determined is the past year 1849.

For all epochs	between	1460 and 1520	multiply	the interval	by	17′.
...	...	1520 ... 1580	...	...		16′.
...	...	1580 ... 1640	...	...		15′.
...	...	1640 ... 1700	...	...		14′.
...	...	1700 ... 1760	...	...		13′.
...	...	1760 ... 1820	...	...		12′.
...	...	1820 ... 1880	...	...		11′.

This table is only for the north magnetic pole; in all cases the multiplying number for the south magnetic pole must be one-half the multiplying number for the opposite pole. The mean positions of the two magnetic poles for the fixed epoch 1849, are as follows:—

* Grover's memoir, Orbital Motion of the Magnetic Pole round the North Pole of the Earth, read at the Nineteenth Annual Meeting of the British Association held at Birmingham, 1849.

North magnetic pole, lat. 70° 0′ : west long. 91° 0′.
South magnetic pole, lat. 75° 5′ : east long. 155° 0′.

In taking the epoch of 1492, we shall have an interval of 357 years, which, multiplied by the rate of revolution in longitude of each magnetic pole for that epoch, namely, $16\frac{1}{2}$ minutes annually for the northern, and $8\frac{1}{4}$ minutes annually for the southern, will give 98° 10′ for the former, and 49° 5′ for the latter: hence, if the magnetic poles be removed backwards in west longitude by so much as the amounts stated above, we shall then have their relative positions for the year 1492 as follows:—

1849, N. M. pole, W. long.	91° 0′ :	S. M. pole, E. long.	155° 0′
	98° 10′		49° 5′
1492, N. M. pole, W. long.	189° 10′ :	S. M. pole, E. long.	105° 55

This preliminary is all which is necessary for the adaptation of our method to any given epoch, past or to come. In the present case it is applied to one of the most memorable in nautical astronomy—the discovery of the line without magnetic variation. The merit of this discovery belongs to Christopher Columbus, who crossed it whilst on his celebrated first voyage for the discovery of a western route to the Indies. We think it would be advisable for the Committee of Magnetical Observation to make this epoch the test of all methods proposing to compute magnetic declination; for the question naturally presents itself—how shall we best prove a method but by its application to the recorded observations of the past; and what observation is more memorable than the one here proposed? This eminent navigator has recorded the position of this line in the year of his first voyage, September 25, 1492, in the following words:—

"$2\frac{1}{2}$° west of the island of Corvo, the magnetic variation changed and passed from N.E. to N.W."

We propose to test our method by trying *whether it will find this line in the same spot at the time stated.* An explanation of the process having been fully stated in the former article, it is useless to give a repetition in the present instance. It will be observed, however, that corresponding items with those previously given are here arranged under corresponding numbers.

Mean Declination of the Compass Needle on the Atlantic during the voyage of Christopher Columbus *for the epoch of Sept.* 25, 1492.

(1.) Lat. and long. of position.

N. lat. 27° 0′ : comp. 63°.
W. long. 31° 30′.

(2.) Lat. and long. of N. magnetic pole.

N. lat. 70° : comp. 20°.
W. long. 91°+98° 10′−31° 30′=157° 40′.

(3.) 157° 40′−90°=67° 40′.

sine 20° 0′	953405
sine 67° 40′	996614
sine 18° 27′	950019

(4.) Lat. and long. of S. magnetic pole.

S. lat. 75° 5′ : comp. 14° 55′.
E. long. 155°−49° 5′+31° 30′=137° 25′.

(5.) 137° 25′−90°=47° 25′.

sine 14° 55′	941063
sine 47° 25′	986705
sine 10° 56′	927768

(6.) 63°+18° 27′=81° 27′.

(7.) 81° 27′×20″=27′ 9″
20° 0′× 1′=20′ 0″
47′ 9″

(8.) 180°+18° 27′+10° 56′=209° 23′.

(9.) 32400 : 43841 : : 47′ 9″ : 63′ 47″.
63′ 47″×81° 27′÷10°=8° 51′.
20°+8° 51′=28° 51′.

(10.) 4900° : 110° : : 729° : 16° 21′.
90° : 16° 21′ : : 8° 51′ : 1° 36′.
157° 40′−1° 36′=156° 4′ : comp. 23° 56′.

(11.)

sine 63° 0′	994988
sine 23° 56′	960818
sine 21° 12′	955806

sine of comp. and rad.	23° 56′ . .	1996095
tang. to comp	63° 0′ . .	970717
tang.	60° 52′ . .	1025378

60° 52′+28° 51′=89° 43′.

sine to comp. 21° 12′	996957
sine to comp. 89° 43′	769417
sine to comp. 89° 44′	766374

(12.) sine 28° 51′ 968351
sine 23° 56′ 960818

1929169
sine 89° 44′ 1000000

sine 11° 17′ 929169

We must now find the sides and angles for the southern hemisphere.

(6.) $63° - 10°\ 56' = 52°\ 4'$.

(7.) $52°\ 4' \times 20'' = 17'\ 21''$
$14°\ 55' \times 1' = 14'\ 55''$

$32'\ 16''$

(8.) $180° + 18°\ 27' + 10°\ 56' = 209°\ 23'$.

(9.) $32400 : 43841 :: 32'\ 16'' : 43'\ 39''$.

$43'\ 39'' \times 52°\ 4' \div 10° = 3°\ 47'$.

$14°\ 55' + 3°\ 47' = 18°\ 42'$.

(10.) $5637° : 104°\ 55' :: 727° : 13°\ 31'$.

$90° : 13°\ 31' :: 3°\ 47' : 34'$.

$137°\ 25' + 34' = 137°\ 59'$: comp. $42°\ 1'$.

(11.) sine 63° 0′ 994998
sine 42° 1′ 982565

sine 36° 37′ 977563

sine to comp. and rad. 42° 1′ . . . 1987096
tang. to comp. . . . . 63° 0′ . . . 970717

tang. . . . . 55° 33′ . . . 1016379

$55°\ 33' - 18°\ 42' = 36°\ 51'$.

sine to comp. 36° 37′ 990452
sine to comp. 36° 51′ 990320

sine to comp. 50° 2′ 980772

The comp. of 180° is 129° 58′.

(12.) sine 18° 42′ 950598
sine 42° 1′ 982565

1933163
sine 50° 2′ 988447

sine 16° 15′ 944716

	Sides.	Angles.
(13.)	89° 44′	11° 17′
	129° 58′	16° 15′

219° 42′ : 27° 32′ : : 89° 44′ : 11° 14′.

11° 17′
11° 14′

3′ W. D.,

which is the mean west declination on the Atlantic for the year 1492. It may be objected—Columbus affirms there was no declination, whilst your method gives 3 minutes. Suppose we grant this, it is only an error of 3 minutes in 357 years, or $\frac{1}{2}$ a second annually; an amount so small, that in ordinary practice even the whole would be scarcely noticed. But, reader, this error does not exist. These three minutes show the *mean* declination for the whole year; in other words, it is the declination for about the middle of the year—say *June*: and as the diminution was then going on at the rate of sixteen and a half minutes per year, *the three minutes would just have vanished at the close of the month of September*. Accordingly Columbus discovered the line *when passing this spot on the 25th of September*. Hence there is not even a second of difference between the observation and our calculation, though it involves so great an interval as 357 years. The next illustration is for the epoch of 1722, involving an interval of 127 years. In this instance we shall not go through the whole computation, but merely state the result as given in item 13. It is as follows:—

Sides.	Angles.
52° 30′	26° 51′
152° 41′	22° 7′

205° 11′ : 48° 58′ : : 52° 30′ : 12° 31′.

26° 51′
12° 31′

14° 20′ W. declination,

which is the mean declination for London, 1722.

In the observations of Graham, inserted in the Philosophical Transactions, No. 383, p. 96, for 1724, the mean declination is the same as given in our computation, namely 14° 20′ west.

As before observed, the epochs we have taken are as memorable as any in the history of terrestrial magnetism, and are as widely distant from each other, and from the present,

as any which can be selected for the trial we propose for all methods computing magnetic declination. I would therefore respectfully suggest to the Magnetical Committee of Observation, that every method be required to fulfill the demands of this test.

Manchester, Feb. 15, 1850.

XXVI. *On the Theory of the Tides.* *By the* Rev. Brice Bronwin.

[Continued from vol. xxxv. p. 345.]

AT the close of my last paper on the Theory of the Tides, I expressed an intention of examining the terms of the second order, considering that there might be some among them which might produce a sensible effect. This I have now done, but do not find any so large as I had anticipated; still they may be sufficiently large to have a sensible effect on the largest of the variable terms in the coefficients, and ought therefore to be noticed.

Neglecting quantities of the third order, and those of the second where s enters, and also putting $\alpha=1$, $\rho=1$, we have *Méc. Cél.*, book 1. chap. 8, No. 35,

$$6'=1+\frac{ds}{dr}+\frac{du}{d\theta}+\frac{dv}{d\varpi}+\frac{du}{d\theta}\frac{dv}{d\varpi}-\frac{du}{d\varpi}\frac{dv}{d\theta}.$$

Also

$$\sin\theta'=\sin(\theta+u)=\sin\theta+u\cos\theta-\frac{1}{2}u^2\sin\theta,\quad r'=r+s.$$

These values being substituted in

$$6'r'^2\sin\theta'=r^2\sin\theta,$$

neglecting the same quantities as before, and leaving out the term $\frac{d(r^2s)}{dr}$, we find

$$\frac{du}{d\theta}+\frac{dv}{d\varpi}+\frac{u\cos\theta}{\sin\theta}+\frac{du}{d\theta}\frac{dv}{d\varpi}-\frac{du}{d\varpi}\frac{dv}{d\theta}+u\frac{du}{d\theta}\frac{\cos\theta}{\sin}$$
$$+u\frac{dv}{d\varpi}\frac{\cos\theta}{\sin\theta}-\frac{1}{2}u^2=0.$$

Such is now the equation of continuity; in the terms of the first order change u and v into $u+\Delta u$ and $v+\Delta v$. With these values, leaving out the resulting terms of the first order, the last equation gives

$$\left.\begin{aligned}&\frac{d\Delta u}{d\theta}+\frac{d\Delta v}{d\varpi}+\frac{\Delta u\cos\theta}{\sin\theta}+\frac{du}{d\theta}\frac{dv}{d\varpi}-\frac{du}{d\varpi}\frac{dv}{d\theta}+\frac{u\cos\theta}{\sin\theta}\\&\left(\frac{du}{d\theta}+\frac{dv}{d\varpi}\right)-\frac{1}{2}u^2=0.\end{aligned}\right\}\ (1.)$$

In like manner we shall have in the value of $\delta\omega$ the terms

$$\left.\begin{aligned}\delta\omega=\delta\theta\left\{\frac{d^2\Delta u}{dt^2}-2n\sin\theta\cos\theta\frac{d\Delta v}{dt}\right\}+\delta\varpi\\ \left\{\sin^2\theta\frac{d^2\Delta v}{dt^2}+2n\sin\theta\cos\theta\frac{d\Delta u}{dt}\right\}\end{aligned}\right\} \quad . \quad . \quad (2.)$$

In these formulæ Δu and Δv are the parts of u and v depending on the terms of the second order. To the last of them must be added the terms depending on the powers and products of u and v.

It will be convenient to develope the formula (F) No. 32 of the chapter before referred to anew. For this purpose let

$$\rho=r+s, \quad \varphi=\theta+u, \quad \psi=\varpi+v;$$

then we have

$$x=\rho\cos\varphi, \quad y=\rho\sin\varphi\cos(nt+\psi), \quad z=\rho\sin\varphi\sin(nt+\psi).$$

These values being substituted in (F), neglecting $\frac{d\rho}{dt}, \frac{d^2\rho}{dt^2}$, and $\delta\rho$ where it is multiplied by the displacements, these quantities being exceedingly small compared with those retained, we find

$$\delta x\frac{d^2x}{dt^2}+\delta y\frac{d^2y}{dt^2}+\delta z\frac{d^2z}{dt^2}=\rho^2\delta\varphi\left\{\frac{d^2\varphi}{dt^2}-2n\sin\varphi\cos\varphi\frac{d\psi}{dt}\right.$$
$$\left.-\sin\varphi\cos\varphi\frac{d\psi^2}{dt^2}\right\}+\rho^2\delta\psi\left\{\sin^2\varphi\frac{d^2\psi}{dt^2}+2n\sin\varphi\cos\varphi\frac{d\varphi}{dt}\right.$$
$$\left.+2\sin\varphi\cos\varphi\frac{d\varphi}{dt}\frac{d\psi}{dt}\right\}-\frac{n^2}{2}\delta(\rho^2\sin^2\varphi).$$

At the surface in the state of equilibrium the last term of the preceding equation becomes

$$-\frac{n^2}{2}\delta(r^2\sin^2\varphi),$$

which is to be subtracted, as we shall perceive, if we consider the mode of procedure with reference to terms of the first order only. But $\rho=r+h$, $\rho^2-r^2=2rh$, neglecting h^2 as being insensible. We may therefore make

$$\frac{n^2}{2}\delta\Big((\rho^2-r^2)\sin^2\varphi\Big)=n^2\delta(rh\sin^2\varphi)=0,$$

since rn^2h is very small compared with gh. And thus, if we make $\rho=r=1$ as heretofore, we have

$$\delta\omega=\delta\varphi\left\{\frac{d^2\varphi}{dt^2}-2n\sin\varphi\cos\varphi\frac{d\psi}{dt}-\sin\varphi\cos\varphi\frac{d\psi^2}{dt^2}\right\}$$
$$+\delta\psi\left\{\sin^2\varphi\frac{d^2\psi}{dt^2}+2n\sin\varphi\cos\varphi\frac{d\varphi}{dt}+2\sin\varphi\cos\varphi\frac{d\varphi}{dt}\frac{d\psi}{dt}\right\}.$$

We must now substitute for φ and ψ their values $\theta+u$ and $\varpi+v$;

$$\sin\varphi=\sin\theta+u\cos\theta,\quad \cos\varphi=\cos\theta-u\sin\theta;$$

then, leaving out the terms of the first order and retaining those of the second only, we shall find

$$\delta\Delta\omega=\delta\theta\left\{(\sin^2\theta-\cos^2\theta)2nu\frac{dv}{dt}-\sin\theta\cos\theta\frac{dv^2}{dt^2}\right\}$$
$$+\delta\varpi\left\{(\cos^2\theta-\sin^2\theta)2nu\frac{du}{dt}+2\sin\theta\cos\theta\left(u\frac{d^2v}{dt^2}+\frac{du}{dt}\frac{dv}{dt}\right)\right\}$$
$$-\delta u\left\{\frac{d^2u}{dt^2}-2n\sin\theta\cos\theta\frac{dv}{dt}\right\}+\delta v$$
$$\left\{\sin^2\theta\frac{d^2v}{dt^2}+2n\sin\theta\cos\theta\frac{du}{dt}\right\}\quad\ldots\ldots\quad(3.)$$

where in the general case

$$\delta u=\frac{du}{d\theta}\delta\theta+\frac{du}{d\varpi}\delta\varpi,\quad \delta v=\frac{dv}{d\theta}\delta\theta+\frac{dv}{d\varpi}\delta\varpi;$$

but for those terms which do not contain ϖ,

$$\delta u=\frac{du}{d\theta}\delta\theta,\quad \delta v=\frac{dv}{d\theta}\delta\theta.$$

We shall make

$u_2=A\cos 2(\varphi-\epsilon_2)$, $v_2=B\sin 2(\varphi-\epsilon_2)$, $u_1=C\cos(\varphi-\epsilon_1)$, $v_1=D\sin(\varphi-\epsilon_1)$;

also

$$u_0=\sigma,\quad v_0=\tau:$$

these two last containing only terms of long periods, and in the first four φ being put for $nt+\varpi-\psi$.

Since $r=1$,

$$\frac{n^2}{g^2}=\frac{1}{289}.$$

If also $g=1$, then

$$n^2=\frac{1}{289}.$$

We must therefore consider n as being a very small quantity, and ν, the mean motion of the planet, is much less. Consequently we must neglect such terms as

$$n\frac{d\sigma}{dt},\ n\frac{d\tau}{dt},\ \frac{d^2\sigma}{dt^2},\ \frac{d^2\tau}{dt^2},\ \frac{d\sigma^2}{dt^2},\ \frac{d\tau^2}{dt^2},$$

as they contain the factors $n\nu$ or ν^2. But though these quan-

tities are insensible, σ and τ may be very considerable, since the terms depending on u_2, v_2, u_1, v_1 may be negative portions of the displacements.

We now proceed to find the terms depending upon the argument $2(\phi-\epsilon_2)$, and first as they result from u_1, v_1.

From the first paper of this series we have

$$C=-\frac{A_1}{n}=-\frac{a_1}{n},\quad D=\frac{B_1}{n\sin\theta}=\frac{a_1\cos\theta}{n\sin\theta}.$$

Substituting in (3.) the values of u_1 and v_1, there result

$$\left.\begin{aligned}&-\frac{3}{2}a^2{}_1\sin\theta\cos\theta\delta\theta+a^2{}_1\delta\theta\left\{-\frac{3}{2}\sin\theta\cos\theta+\frac{\cos\theta}{\sin\theta}\right\}\\&\quad\cos 2(\phi-\epsilon_1)+a^2{}_1\delta\varpi\left(1-\frac{1}{2}\sin^2\theta\right)\sin 2(\phi-\epsilon_1)\end{aligned}\right\} \quad . \quad (4.)$$

If we turn to the second paper of this series, we find

$$D_1=na_1\sin\theta\cos\theta.$$

But D_1 is the coefficient of $\sin(\phi-\epsilon_1)$ in the value of $\frac{\delta\omega}{\delta\varpi}$ Now $n=\frac{1}{17}$; but if we consider for a moment the height of the tide at any place whatever, we must conclude that a_1 is very much less than this quantity, and therefore that the above terms, including $a^2{}_1$ in their coefficients, may be neglected.

In finding the terms which result from combining u_2, v_2 with u_0, v_0, we must observe that the terms of σ and τ do not contain ϖ, and that we must not employ the differentials of these quantities relative to the time; thus we shall easily find these terms to be

$$\begin{aligned}&\theta(\sin^2\theta-\cos^2\theta)4n^2B\sigma\cos 2(\phi-\epsilon_2)+\delta\varpi\{(\sin^2\theta-\cos^2\theta)\\&\quad 4n^2A\sigma-8n^2\sin\theta\cos\theta B\tau\}\sin 2(\phi-\epsilon_2)-\delta\theta(A+\sin\theta\cos\theta B)\\&4n^2\frac{d\sigma}{d\theta}\cos 2(\phi-\epsilon_2)-\delta\theta(\sin^2\theta B+\sin\theta\cos\theta A)4n^2\frac{d\tau}{d\theta}\sin 2(\phi-\epsilon_2).\end{aligned}$$

From the first paper of this series we have by comparison,

$$A=-\frac{1}{2n}A_2=-\frac{a_2}{2n}\left(\frac{\sin\frac{\theta}{2}}{\cos^3\frac{\theta}{2}}+\frac{2\sin\frac{\theta}{2}}{\cos\frac{\theta}{2}}\right),$$

$$B=\frac{B_2}{2n\sin\theta}=\frac{a_2}{4n\cos^4\frac{\theta}{2}}+\frac{a_2}{2n}.$$

By the substitution of these values the coefficients would be

very complicated; but it would be useless to find them as functions of θ, since they contain arbitrary constants which can only be determined by observation; we may therefore write the quantity last found thus,

$$\left.\begin{array}{l}\delta\theta\left(\mathrm{G}\sigma-\mathrm{H}\frac{d\sigma}{d\theta}\right)\cos 2(\phi-\epsilon_2)+\delta\varpi\mathrm{K}\sigma\sin 2(\phi-\epsilon_2)\\ \qquad -\delta\theta\mathrm{L}\frac{d\tau}{d\theta}\sin 2(\phi-\epsilon_2),\end{array}\right\}\quad(5.)$$

where σ and τ are the only variable quantities in the coefficients.

The coefficients here will contain the factor na_2, which is the same as in the like terms of the first order; besides this they will have the factor σ, or $\frac{d\sigma}{d\theta}$, $\frac{d\tau}{d\theta}$. They will probably be insensible in most places, but in some places they may perhaps be as large as the largest of the variable terms in the coefficients of the terms of the first order.

In the equation of continuity, the terms of the second order depending on u_1, v_1 are easily found to be

$$\frac{a^2_1}{4n^2}+\left(\frac{a^2_1}{4n^2}-\frac{a^2_1}{n^2\sin^2\theta}\right)\cos 2(\phi-\epsilon_1), \quad . \quad . \quad (6.)$$

which for the reason before stated may be neglected.

Those depending on u_2, v_2, combined with u_0, v_0, are

$$\left\{2\mathrm{B}\left(\frac{d\sigma}{d\theta}+\sigma\frac{\cos\theta}{\sin\theta}\right)+\frac{\cos\theta}{\sin\theta}\frac{d(\mathrm{A}\sigma)}{d\theta}\right\}\cos 2(\phi-\epsilon_2)+2\mathrm{A}\frac{d\tau}{d\theta}$$
$$\sin 2(\phi-\epsilon_2)-\frac{1}{2}\sigma^2.$$

To abridge, we shall write this

$$-\frac{1}{2}\sigma^2+\left(\mathrm{M}\sigma+\mathrm{N}\frac{d\sigma}{d\theta}\right)\cos 2(\phi-\epsilon_2)+2\mathrm{A}\frac{d\tau}{d\theta}\sin 2(\phi-\epsilon_2). \quad (7.)$$

Collecting all the terms of (2.) and (5.), and adopting new symbols to abridge further, we have

$$\delta\Delta\omega=\delta\theta$$
$$\left\{\frac{d^2\Delta u}{dt^2}-2n\sin\theta\cos\theta\frac{d\Delta v}{dt}+\Upsilon\cos 2(\phi-\epsilon_2)+\Omega\sin 2(\phi-\epsilon_2)\right\}$$
$$+\delta\varpi\left\{\sin^2\theta\frac{d^2\Delta v}{dt^2}+2n\sin\theta\cos\theta\frac{d\Delta u}{dt}+\Psi\sin 2(\phi-\epsilon_2)\right\}.$$

But since Δu and Δv are of the form

$$\mathrm{P}\sin 2(\phi-\epsilon_2)+\mathrm{Q}\cos 2(\phi-\epsilon_2),$$

and $\frac{dP}{dt}, \frac{dQ}{dt}$ are far too small to render it necessary to take account of them, it is obvious that

$$\frac{d^2\Delta u}{dt^2} = -n^2\Delta u, \quad \frac{d^2\Delta v}{dt^2} = -n^2\Delta v,$$

since $\varphi = nt +$ a constant and very small variable quantities. Therefore

$$\delta\Delta\omega = \delta\theta \left\{ -n^2\Delta u - 2n\sin\theta\cos\theta\frac{d\Delta v}{dt} + \Upsilon\cos 2(\varphi - \epsilon_2) + \Omega\sin 2(\varphi - \epsilon_2) \right\}$$
$$+ \delta\varpi \left\{ -n^2\sin^2\theta\Delta v + 2n\sin\theta\cos\theta\frac{d\Delta u}{dt} + \Psi\sin 2(\varphi - \epsilon_2) \right\}.$$

That this may be a complete variation, we must have

$$\frac{d}{d\varpi}\left\{ -n^2\Delta u - 2n\sin\theta\cos\theta\frac{d\Delta v}{dt} + \Upsilon\cos 2(\varphi - \epsilon_2) + \Omega\sin 2(\varphi - \epsilon_2) \right\}$$
$$= \frac{d}{d\theta}\left\{ -n^2\sin^2\theta\Delta v + 2n\sin\theta\cos\theta\frac{d\Delta u}{dt} + \Psi\sin 2(\varphi - \epsilon_2) \right\}.$$

But if we attend to the value of φ, we see that

$$\frac{d\Delta u}{d\varpi} = \frac{1}{n}\frac{d\Delta u}{dt}, \quad \frac{d^2\Delta v}{d\varpi dt} = \frac{1}{n}\frac{d^2\Delta v}{dt^2} = -n\Delta v.$$

Also for the quantities we are seeking we may put

$$\frac{d\Delta v}{d\theta} = k\Delta v, \quad \frac{d^2\Delta u}{d\theta dt} = l\frac{d\Delta u}{dt},$$

since the coefficients only are affected by the operation $\frac{d}{d\theta}$. The preceding equation therefore, when reduced, may be written,

$$\left. \begin{array}{l} K_1\frac{d\Delta u}{dt} + K_2\frac{d\Delta v}{dt} + K_3\Delta v + K_4\sin 2(\varphi - \epsilon_2) \\ + K_5\cos 2(\varphi - \epsilon_2) = 0. \quad . \quad . \quad . \quad . \quad . \quad . \end{array} \right\} \quad . \quad (8.)$$

Leaving out the term $-\frac{1}{2}\sigma^2$, if we put the other terms of (7.) in (1.), the result may be written

$$\frac{d\Delta u}{d\theta} + \frac{d\Delta v}{d\varpi} + \frac{\Delta u\cos\theta}{\sin\theta} + \Gamma\cos 2(\varphi - \epsilon_2) + \Lambda\sin 2(\varphi - \epsilon_2) = 0.$$

And this may be put under the more simple form,

$$L_1\Delta u + \frac{1}{n}\frac{d\Delta v}{dt} + \Gamma\cos 2(\varphi - \epsilon_2) + \Lambda\sin 2(\varphi - \epsilon_2) = 0 \quad . \quad (9.)$$

If we differentiate (8.) and (9.) relative to t, and put

$$\frac{d^2\Delta u}{dt^2} = -n^2\Delta u, \quad \frac{d^2\Delta v}{dt^2} = -n^2 v,$$

we shall have two other equations; and from the four we easily find the values of

$$\Delta u, \quad \Delta v, \quad \frac{d\Delta u}{dt}, \quad \frac{d\Delta v}{dt},$$

which will all be of the form

$$\mathrm{P} \sin 2(\phi - \epsilon_2) + \mathrm{Q} \cos 2(\phi - \epsilon_2).$$

These being put in the value of $\delta\Delta\omega$, and the integration effected relative to ϖ, we shall have

$$\Delta\omega = \mathrm{P}_1 \sin 2(\phi - \epsilon_2) + \mathrm{Q}_1 \cos 2(\phi - \epsilon_2), \quad . \quad . \quad (10.)$$

where P_1 and Q_1 are of the order $na_2\sigma$, $na_2\tau$.

In order to add these terms to those of the first order, we must change, in page 265 of the second paper, $\mathrm{D}_2 \cos 2(\phi - \epsilon_2)$ into

$$\mathrm{D}_2 \cos 2(\phi - \epsilon_2) + \mathrm{D}_3 \sin 2(\phi - \epsilon_2),$$

and then the first and second equations (12.) in the following page will become

$$\left.\begin{aligned} \mathrm{F}_2 \cos 2\beta_2 &= \mathrm{D}_2 \cos 2\epsilon_2 + \mathrm{E}_2 \rho^3 \cos^2 v - \mathrm{D}_3 \sin 2\epsilon_2 \\ \mathrm{F}_2 \sin 2\beta_2 &= \mathrm{D}_2 \sin 2\epsilon_2 + \mathrm{D}_3 \cos 2\epsilon_2. \quad . \quad . \quad . \end{aligned}\right\}. \quad (11.)$$

But here, it must be remembered, D_2 is changed from its former value by the addition of small terms containing σ and τ, and D_3 is of the order $na_2\sigma$ or $na_2\tau$, as it contains these in all its terms.

By the process employed in the paper just now referred to, we find

$$\mathrm{F}_2 = \mathrm{D}_2 + \mathrm{E}\rho^3 \cos^2 v \cos 2\epsilon_2. \quad . \quad . \quad . \quad . \quad (12.)$$

Also

$$\Delta = -\frac{\mathrm{E}_2}{2\mathrm{D}_2} \rho^3 \cos^2 v \sin 2\epsilon_2 + \frac{\mathrm{D}_3}{2\mathrm{D}_2},$$

and thence

$$\beta_2 = \epsilon_2 - \frac{\mathrm{E}_2}{2\mathrm{D}_2} \rho^3 \cos^2 v \sin 2\epsilon_2 + \frac{\mathrm{D}_3}{2\mathrm{D}_2}. \quad . \quad . \quad (13.)$$

But, as before observed, D_2 is not exactly the same here as in that paper. In (18.) of the same paper we must add to the value of β_2 the small term $\frac{\mathrm{D}_3}{2\mathrm{D}_2}$, and the quantities D_2, D_3 require to be developed.

The quantities σ and τ are those parts of the values of u and v which do not contain ϕ, and may therefore be supposed

to arise from the first term of (1.) of the first paper, and to be of the form $\frac{1}{r^3}(a+b\sin^2 v)$; but as they are multiplied by small quantities in the values of D_2, D_3, we may neglect the part of r depending on e. A constant resulting from integration should be added, and may be supposed to be included. We may therefore change in (18.) D_2 into $D_2+m\sin^2 v$, or K_2 into $K_2+m\sin^2 v$, D_2 being now constant, and make

$$m-E_2\cos 2k_2=L_2.$$

Then we must change G_2 into

$$G_2+\frac{mE_2}{2D^2_2}\sin 2k_2\sin^2 v.$$

Thus we may replace (18.) by

$$F_2=K_2+L_2\sin^2 v-\frac{1}{2}E_2\sin^2 o\sin 2k_2\sin 2z+3eE_2\cos 2k_2$$
$$\cos(z-\pi)+4eE_2\sin 2k_2\sin(z-\pi).$$
$$\beta_2=G_2+N_2\sin^2 v-\frac{1}{4}H_2\sin^2 o\sin z-2eH_2\sin(z-\pi)$$
$$-\frac{3eE_2}{2D_2}\cos(z-\pi). \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (14.)$$

Here $\frac{D_3}{2D_2}$ has been made equal to

$$\frac{p+q\sin^2 v}{2D_2},$$

and we have

$$G_2=k_2-\frac{E_2}{2D_2}\sin 2k_2+\frac{p}{2D_2},\ N_2=\left(1+\frac{m}{D_2}\right)\frac{E_2}{2D_2}\sin 2k_2+\frac{q}{2D_2}.$$

Gunthwaite Hall, near Barnsley, Yorkshire,
February 4, 1850.

[To be continued.]

XXVII. *On the Nitroprussides, a New Class of Salts.* *By* Dr. Lyon Playfair, *F.R.S., F.C.S.**

1. In an inquiry into the constitution of the prussides, I found it necessary to examine into the somewhat anomalous action of nitric acid on the yellow prusside of potassium. This examination has led to the discovery of a singular class of compounds, which form the subject of the present memoir.

The previous knowledge on the action of nitric acid on the prussides may be summed up very briefly. Thomson* examined the gases produced during the action, and recognized

* From the Philosophical Transactions for 1849, part ii.; having been received by the Royal Society June 21, and read June 21, 1849.

† As quoted by Gmelin, *Handbuch*, vol. iv. p. 370.

them to be nitrogen, cyanogen, nitric oxide, and carbonic acid, while the residue was believed to consist of pernitrate of iron and nitrate of potash. Döbereiner* remarked that previous to the complete decomposition of the prussides, a strong coffee-coloured liquid was produced, which, after neutralization, precipitated protosalts of iron of a dark blue colour. Gmelin†, to whom chemistry was already indebted for important discoveries in the prussides, observed that the coffee-coloured liquid noticed by Döbereiner was rendered of a magnificent purple or blue colour on the addition of an alkaline sulphide. The same fact was noted by Mr. Mercer‡ of Oakenshaw, without his being aware that it had already been remarked by Gmelin. Campbell§, in repeating Gmelin's experiment, threw out the intelligent suggestion that the purple colour might be due to the production of a sulphuret of nitrogen, which Gregory|| had already remarked produced an amethystine colour when mixed with an alcoholic solution of potash. Smee¶, in an examination of the action of nitric acid on the prussides, observes that ferridcyanide is produced, nitric oxide being evolved.

I am not aware of any further knowledge on this subject; and as it is far from being sufficiently extended, a new examination was desirable.

2. When dissolved ferrocyanide of potassium is digested with diluted nitric acid, a coffee-coloured liquid is produced, having the characters ascribed to it by Döbereiner and Gmelin. The addition of this acid solution to sulphide of potassium dissolved in water causes a precipitation of sulphur and the production of various colours, from a pink to a violet or blue shade. When the acid liquid is neutralized with potash, it immediately produces the most intense purple coloration with a soluble sulphide**. The action of nitric acid on the pounded salt is similar, but much more violent than that experienced with the solution. Nitric oxide is at first evolved, but it soon ceases if the mixture be kept cool, and it is followed by the copious escape of cyanogen gas, accompanied by hydrocyanic acid, and a gas of peculiar pungency, apparently hydrated cyanic acid; more or less nitrogen and carbonic acid are also

* Schw. J. xxvi. p. 305.

† *Ann. Pharm.* vol. xxviii. p. 57, and Memoirs of Chem. Soc. vol. i. p. 41.

‡ Unpublished Letter.

§ *Handbuch*, vol. i. p. 167.

|| Turner's Chemistry, p. 343.

¶ Phil. Mag. vol. xvii. 194.

** The intensity and beauty of this coloration render the nitroprussides the most sensible of all tests for the presence of the minutest trace of a soluble sulphide. The presence of quantities insensible to ordinary tests is at once strongly exhibited by the use of this colouring agent.

found in the escaping gases. The dark red solution remaining after the action, deposits, on cooling, abundance of nitrate of potash, and, under the most favourable circumstances, about 5 per cent. of a peculiar *white substance*, afterwards to be described. The red-coloured solution now precipitates protosalts of iron of a dark blue colour, or if it has been heated for a short time, or even stood in the cold for some days, of a dark green, and sometimes of a slate colour. A dark green precipitate is also produced on the addition of salts of copper. The same precipitates are obtained from the neutralized as from the acid solution. Such were the preliminary observations made on repeating Döbereiner's experiment.

One important fact was observed in this preliminary trial, viz. that nitric oxide disappeared during the action, and in fact only occurred when the transformation was so violent as to escape control. This gas was therefore probably one important cause of the change, and it therefore became necessary to examine its action on the cyanides, as a more simple means of eliciting its mode of action.

3. The first obvious experiment was to ascertain whether cyanide of potassium charged with nitric oxide would produce prussides exerting the remarkable colouring action on the sulphides. Nitric oxide is in fact readily absorbed by cyanide of potassium, the solution becoming red-coloured and depositing a black substance resembling paracyanogen. This red-coloured solution did not of itself give any colour when mixed with a sulphide. It was now converted into a prusside by the addition of protosulphate of iron. The resulting prusside was now found to strike a magnificent purple colour with a soluble sulphide. The same coloration was obtained when a prusside was made from common cyanide of potassium added to a solution of protosulphate of iron, through which nitric oxide had been passed. It was obvious from these experiments that nitric oxide was one of the great causes of the change experienced by the prusside.

4. The action of nitric oxide on the prussides themselves was now examined. It was found that nitric oxide could be passed through a solution of ferrocyanide of potassium without producing any sensible change. But when the prusside was mixed with sufficient acid to take up its alkaline base, it was now found that nitric oxide was freely absorbed by this mixture when heated, though not in the cold; and that the resulting liquid exhibited the strong coloration of sulphides. Ferrocyanide of lead, or any other ferrocyanide, gave, when mixed with strong acids, a similar result. It was therefore obvious that the peculiar compound might be obtained from

pure hydroferrocyanic acid. The latter acid was prepared from prusside of lead by sulphuretted hydrogen, the excess of the latter being removed by the addition of a little more lead salt. The filtered ferrocyanic acid was found to suffer no change when exposed to the action of nitric oxide in the cold; but when the solution was kept in a water-bath and the gas led through it, a change was observed. This, however, at first merely consisted in the transformation of ferrocyanic to ferridcyanic acid,—

$$4(Fe\,Cy^3+2H)+NO^2=2(Fe^2\,Cy^6+3H)+2HO+N.$$

Until this change was completed not the least coloration took place on mixing the acid liquid with a sulphide. When, however, the acid no longer gave prussian blue with perchloride of iron, it began to assume a red colour, continuing to evolve a gas, and it now exhibited, after neutralization, the peculiar coloration with sulphides. It now gave a blue precipitate with protosulphate of iron, like ordinary ferridcyanic acid. This blue precipitate became paler in colour as the gas continued to stream through the hot solution, until finally the addition of the iron salt gave a precipitate of a *clear salmon colour*. Here then was the acid* of the new compounds, and its salts were obtained by neutralization with the respective bases. This process was a great step in the inquiry, because it enabled the distinctive characters of the nitroprussides to be determined. At the same time it was not fitted to procure the salts in sufficiently large quantities for examination. By showing however what was to be looked for, it enabled a more complete examination to be made of the products of oxidation of the prussides by nitric acid, with a view to the separation of the nitroprussides from the ferridcyanides, with which they were obviously mixed.

5. It was observed that the oxidized prusside required a very small quantity of protosulphate of iron for its complete precipitation. One double equivalent of ferrocyanide of potassium ($Fe^2\,Cy^6+4K$) was oxidized with 3 equivs. of nitric acid diluted with its own volume of water. The dark red, almost black liquid, was diluted with water and treated with a known quantity of sulphate of iron dissolved in water. Prussian blue was formed, but it remained in *solution*, forming a dark blue soluble fluid, of great beauty and intensity. When the added sulphate of iron amounted to one equivalent, that is to one-fourth of the potassium originally in the prusside, the prussian

* On neutralizing this nitroprussic acid with carbonates, the resulting salts were found to be accompanied by a nitrate, although the nitric oxide had previously been passed through water in a washing-bottle.

blue became insoluble and was thrown on a filter and washed. It was obvious from this experiment that there must be a potassium salt in combination with the prussian blue, because the quantity of iron salt added was quite insufficient to unite with the iron and cyanogen of the radical. This idea was confirmed by finding that 2 equivs. of sulphate of copper were required to effect the precipitation, which 1 equiv. of sulphate of iron had effectually done. To separate the potassium salt present in the latter case, the precipitated prussian blue, after being washed with cold water, was mixed with water and boiled. The whole was now thrown on a filter, and a solution of a fine ruby-red colour passed through. This solution gave a salmon-coloured precipitate with a protosalt of iron. This precipitate does not readily occur in an acid liquid, and hence the addition of the iron salt to the original oxidized solution does not effect a complete precipitation, the filtrate from it being yellow from dissolved nitroprusside of iron. There being always some nitroprusside of iron along with the prussian blue, the simple treatment with hot water does not wholly œconomise the products, as it only separates the salt of potassium. The mixture may therefore be decomposed by caustic potash, which, added in sufficient quantity, forms peroxide of iron, and ferrocyanide instead of ferridcyanide,—

$$(Fe^2Cy^6+3K)+KO+2FeO=2(FeCy^3+2K)+Fe^2O^3.$$

The ferrocyanide may now be separated from the nitroprusside, either by precipitation by alcohol, or by the addition of nitrate of lead. These plans were not however so advantageous as the simple means of separation given above. That method was followed for some time until the examination of the nitroprussides threw some light on their properties and composition; it was then found that a process yielding a much larger product of the new compound might be invented. The following study was therefore made of the products arising from the oxidation of the prussides by nitric acid. The knowledge thus obtained led, as was expected, to a very œconomical and simple means of obtaining the nitroprussides in large quantities.

6. As nitric oxide was one of the most important means of producing the conversion of prussides into nitroprussides, it was necessary to operate so as to prevent its escape. This was done by keeping the mixture of acid and prusside well-cooled at the first part of the action. Nitric oxide is almost always evolved at first, but it soon diminishes to nothing as the action proceeds. A copious evolution of gas takes place. The escaping gas burns with the characteristic purple flame

of cyanogen. Led through protosulphate of iron, after the first violent action has ceased, no blackening is perceived, so that nitric oxide has ceased to be evolved. Led into caustic barytes, carbonate of barytes is precipitated, and the solution is found to contain cyanide of barium and cyanate of barytes. When the gas is collected over mercury and potash is thrown into the tube containing it, a portion of gas still remains unabsorbed and is easily recognized as nitrogen. When the escaping gas is led into water it is dissolved in considerable quantity, and the water now smells strongly of cyanogen and of a peculiar pungent gas, which appears to be hydrated cyanic acid. The gas treated with ammonia deposits azulmic acid, and the usual products of the transformation of cyanogen. The following process is found best adapted for the preparation of the nitroprusside. Nitric acid of commerce is diluted with its own bulk of water, and the quantity of it necessary to neutralize 53·3 grs. of carbonate of soda (1 equiv.) is ascertained by the alkalimeter. This quantity denotes 1 equiv. of acid.

Ferrocyanide of potassium is now reduced to powder and is placed in a convenient vessel, and for every 422 grs. of the salt used (that is for 1 equiv. $Fe^2Cy^6, 4K+6HO$) 5 equivs. of the acid are employed. This quantity of acid is found to produce an œconomical result, but it is very remarkable that one-fifth of the quantity, or 1 equiv., is sufficient to convert a large portion of the prusside into nitroprusside. This is the more remarkable, because there are four available equivalents of potassium, and it was to be expected that nitrate of potash would be produced. This however is not the case, 1 equiv. of nitric acid effecting oxidation to a considerable extent on a double equivalent of yellow prusside. The five equivalents of acid mentioned above are at once poured on the prusside, as the cooling effect of the whole reduces the violence of the action. The mixture assumes a milky appearance, but soon the salt dissolves with a brownish-red colour like coffee, the mixture of gases already described being freely evolved. When the solution is complete, it is found to contain ferridcyanide of potassium mixed with a nitroprusside and nitrate of potash. It is now removed into a bolt-head and digested in the water-bath. It continues to evolve gas, and after a time it no longer yields prussian blue with sulphate of iron, but forms a dark green or a slate-coloured precipitate. The solution is now removed from the water-bath and is allowed to cool, during which abundance of nitrate of potash crystallizes out, and always more or less of a peculiar white substance. The dark coffee-coloured mother-liquor is now neutralized

with carbonate of soda or carbonate of potash, according as salts of sodium or potassium are desired. The neutralized solution shows the presence of iron existing as a base, for prussian blue is precipitated on the addition of a prusside. The neutral solution is now boiled, and it deposits generally a green precipitate, though occasionally one of a brown colour; and the filtrate is found to be of a dark ruby-red, containing only nitroprusside of the base employed and a nitrate. The latter is separated by crystallization in the manner pointed out under the respective salts. Nitroprusside of sodium, being most easily prepared, is recommended as the product of the process here given.

Some practical difficulties may be mentioned so as to prevent disappointment in the preparation. A carbonate of and not the caustic alkali should be employed in the neutralization. When the latter is used, the solution of nitroprusside is apt to be mixed with ferrocyanide. When this takes place an addition of acid serves to remove the impurity, as some of the precipitated oxide of iron is dissolved, and forming prussian blue with the ferrocyanide, removes it from the solution. This impurity may also be removed by the addition of nitrate of lead, which precipitates the prusside but not the nitroprusside; or it may be taken away by the gradual addition of sulphate of iron, which removes the ferrocyanide before precipitating the nitroprusside. When the quantity last added precipitates the solution of a salmon colour, the impurity has been removed*.

Red prusside (ferridcyanide) of potassium may be used in the preparation exactly as described for the yellow prusside.

7. The following experiments were made in order to ascertain approximatively how much nitroprusside was formed by the process now described. 105·5 grs. crystallized ferrocyanide of potassium were digested with $1\frac{1}{3}$ equiv. of nitric acid. After digestion the liquid was neutralized with carbonate of soda and boiled, the resulting green precipitate being collected on a weighed filter. The filtrate was precipitated by a salt of copper, and the nitroprusside of copper was collected and weighed.

It was found by various trials that perfectly uniform results could not be obtained, the amount and even the composition of the precipitate† on boiling varying with the conditions of

* It is perhaps needless to remark, that when the ruby-red solution free from prussides has been obtained by any of the processes above described, it may be used at once for the precipitation of the insoluble nitroprussides.

† The composition of the precipitates varies considerably. If on neutralizing the acid solution an excess of alkali be added, the addition of an

the preparation. The two following experiments may be taken as giving mean results :—

I. 105·5 grs. yellow prusside gave 8·275 green precipitate, yielding on incineration 7·95 grs. peroxide of iron; the filtrate gave 48·90 grs. nitroprusside of copper.

II. 105·5 grs. yellow prusside gave 8·32 grs. green precipitate, yielding by calculation 6·30 grs. peroxide of iron; the filtrate gave 46·12 grs. nitroprusside of copper.

Taking the mean of the two experiments, 105·5 grs., or one-fourth of the double equivalent of yellow prusside, yield 8·297 grs. green precipitate containing 4·984 grs. iron; the filtrate yields 47·51 grs. nitroprusside of copper. But before drawing deductions, it is necessary to know the composition of the green precipitate. It consists of a mixture of prussian blue, nitroprusside and peroxide of iron, this mixture not being constant. However, to take a special case as an example,—

22·26 grs., calcined and treated with nitrate of ammonia, gave 13·62 grs. peroxide of iron, or 42·83 per cent. of metallic iron.

9·49 grs. burned with oxide of copper gave 4·13 grs. carbonic acid and 0·96 gr. water; the carbon is therefore 11·87 per cent., the water 10·11.

35·02 grs. treated by caustic potash, gave, when neutralized by acetic acid, a red filtrate, from which the ferrocyanide of potassium was precipitated by alcohol; the filtrate from this had all the properties of nitroprusside of potassium, and gave by precipitation with sulphate of copper 13·98 grs. nitroprusside of copper, equal to 13·24 grs. nitroprusside of iron, or 37·80 per cent.

The reactions in the preparation of the nitroprusside may now be approximatively explained.

By reference to the ascertained composition of the nitroprussides, it will be seen that the 47·51 grs. of the copper nitroprusside obtained from the 105·5 grs. of yellow prusside, are equal to 35·69 grs. anhydrous nitroprussic acid: this quantity contains 9·66 grs. of iron. Now 14 grs. iron were present in the yellow prusside used, so that about two-thirds

acid gives a slaty precipitate, which consists mainly of oxide of iron mixed with prussian blue. Under somewhat similar conditions, I believe, though on this subject I am not certain, the precipitate on boiling, instead of being green, is brown, like oxide of iron. On washing and exposure to the air it becomes green.

In an experiment where this precipitate came, 105·5 grs. yellow prusside gave 5·83 grs. of a brown precipitate, and 50·66 grs. of nitroprusside of copper. In another experiment with a like quantity, 4·755 grs. of the brown precipitate were obtained.

of the iron have been converted into nitroprussic acid. The other third is in the green precipitate, which was found to contain 4·98 iron; if it had been one-third it should have been 4·66; of this quantity 1·19 is as nitroprusside of iron, and therefore 0·59 as nitroprussic acid. Hence we have out of the 14 grs. iron present in the ferrocyanide 10·25 grs. converted into nitroprussic acid, or very nearly three-fourths; the remaining one-fourth is partly as prussian blue and oxide of iron, and partly as the basic iron in the nitroprusside of iron.

The quantity of carbon or of cyanogen converted into nitroprusside has now to be examined. The 47·51 grs. copper nitroprusside contain 9·93 grs. of carbon, that in the nitroprusside of iron of the green precipitate would amount to 0·60, hence the carbon converted into nitroprussic acid is 10·53. There were 3 equivs. or 18 grs. of carbon in the yellow prusside, of which about $1\frac{3}{4}$ equiv. has been converted into nitroprusside; of the remaining $7\frac{1}{2}$ grs. carbon or 16·2 grs. cyanogen, about 0·38 gr. carbon or 0·823 gr. cyanogen remain in the green precipitate as a cyanide, the remainder escaping as a gas. It is true that the results here given only form a rude approximation, but they denote sufficiently the final, though not all the intermediate changes which occur; the ultimate action may be expressed by the following equation:—

$$8(FeCy^3+2K)+19(HO, NO^5)=16(KO, NO^5)+(Fe^5\,Cy^{12}\,3NO+5H)+FeCy+Fe^2\,O^3+2H\,Cy+9Cy+12HO.$$

Thus 8 equivs. ferrocyanide of potassium lose their potash by 16 equivs. of nitric acid, and the hydroferrocyanic acid formed is oxidized at the expense of 3 equivs. nitric acid, the 3 equivs. of nitrous oxide thus formed entering into the constitution of nitroprussic acid, 12 equivs. of water being formed by the oxidation. Of the cyanogen, 12 equivs. remain in the nitroprussic acid, 2 equivs. escape as hydrocyanic acid, 9 equivs. as cyanogen, and 1 equiv. remains united with iron as a cyanide. This scheme would require 10·04 grs. of the iron experimented on to be converted into nitroprussic acid, and direct experiment gave 10·2 grs. We should indeed find 1·8 gr. cyanogen in the cyanide of iron*, whereas only 0·823 gr. cyanogen was found in this state; but when we consider the small quantity present and the variable nature of prussian blues, such a discordance is not fatal to the correctness of an explanation, which is only given as an approximation.

8. It has already been mentioned that carbonic acid was

* The empirical formula Fe Cy represents the actual proportion of iron and cyanogen in certain prussian blues, although the elements are not arranged according to this simple expression.

one of the products evolved as a gas. This acid scarcely appears at all when the quantity of nitric acid used is only 1 equiv. for every 4 equivs. of potassium in the prusside. On the contrary, it is a very marked product when 5 equivs. are employed. Precisely under the same circumstances that the carbonic acid is least in quantity, does the peculiar white substance, already referred to, augment; and when the carbonic acid is greatest, as when five equivalents of nitric acid are used, then scarcely any of the white substance is observed. The carbonic acid is therefore obviously a product of the oxidation of the white substance. Five per cent. of the white substance were obtained when one equivalent of nitric acid was used to oxidize an amount of yellow prusside containing 4 equivs. of potassium; to ensure this, the largest quantity obtained by experiment, the action of the acid on the prusside must be as subdued as possible. The white substance is found with the nitrate of potash, which has deposited from the oxidized liquid, and is separated from it by the solution of the latter in water. The white substance is scarcely at all soluble in cold water, and therefore may be collected and purified by repeated solutions in boiling water, in which it is only very sparingly soluble, and deposits itself, on cooling of the solution, as a white crystalline precipitate. It may also be sublimed without change between two watch-glasses. The following analyses of this white substance show its composition. Analyses I. II. were made upon a specimen purified by sublimation; III. IV. upon a specimen purified by solution.

I. 5·05 grs. gave 5·004 grs. carbonic acid and 2·094 grs. water.
II. 7·835 grs. gave 7·85 grs. CO^2 and 3·236 grs. HO.
III. 5·947 grs. gave 5·95 grs. CO^2 and 2·46 grs. HO.
IV. 6·992 grs. gave 6·95 grs. CO^2 and 2·886 grs. HO.

The nitrogen analyses were made by Will and Varrentrapp's plan, the portions used in analysis being in one case purified by sublimation and in the other by solution:—

4·345 grs. gave 21·835 grs. platinum salt.
7·027 grs. gave 35·74 grs. platinum salt.

	Purified by sublimation.		Purified by solution.				Calculated.
C	27·024	27·324	27·255	27·108	2	12	27·27
N	31·583	31·583	31·961	31·961	1	14	31·81
H	4·607	4·589	4·594	4·586	2	2	4·54
O	36·786	36·584	36·190	36·345	2	16	36·38
	100·000	100·000	100·000	100·000			100·00

The carbon is to the nitrogen as 2 : 1, or in the same proportion as cyanogen. In fact the formula $Cy + 2HO$ correctly represents the composition, and the substance may be supposed to be formed by the union of cyanogen in its nascent state with 2 equivs. of water. When this white substance is treated with acids, it is converted into oxalic acid and ammonia. This fact, together with the analysis, proves it to be *Oxamide**. Its occurrence in a process of oxidation is very surprising, and perhaps may throw some doubts on the theoretical composition ascribed to it, $2CO + NH^2$. There is little doubt that this substance is the same as that observed by Vauquelin† in a watery solution of cyanogen, which however was not analysed by him. The description which he gives applies closely to oxamide. Wöhler‡ also observed two substances in a watery solution of cyanogen, one of which may be this body. The appearance of carbonic acid is now explained, as it is obviously due to an oxidation of the oxalic acid produced by the transformation of the oxamide.

SECTION II.—*General remarks on the Nitroprussides.*

9. The nitroprussides are salts with characters so decided, that they cannot be confounded with any known series of compounds. They are generally highly coloured—the salts of potassium, ammonium, sodium, barium, calcium and lead being of a dark red or ruby colour; they are readily soluble in water, and communicate a dark red colour to the solution. Alcohol does not precipitate these salts from their solutions. The soluble nitroprussides crystallize readily, yielding large and well-defined crystals. The nitroprussides of copper, zinc, iron, nickel, cobalt and silver, are either wholly or nearly insoluble.

The following table exhibits some of the characteristic reactions of a soluble nitroprusside:—

Reagents.	Behaviour of the nitroprusside.
Sulphides of the alkaline metals...	Magnificent transitory purple colour.
Sulphuretted hydrogen	Produces prussian blue, a prusside and peculiar compound.
Neutral salts of lead	No change.
Basic salts of lead	White precipitate, after a time in strong solution.
Persalts of mercury	No change.

* In the descriptions of oxamide, it is usual to state that all acids convert it into oxalic acid and ammonia. It is however very readily soluble in concentrated sulphuric acid, from which it is again precipitated unchanged by the addition of water.

† *Ann. de Chim. et de Phys.*, ix. 113; xxii. 132.

‡ Poggendorff's *Annalen*, xv. 627.

Reagents.	Behaviour of the nitroprusside.
Proto- and persalts of tin	No change.
Salts of zinc	Light salmon-coloured precipitate.
Salts of copper	Light green precipitate.
Salts of nickel	Dirty white precipitate.
Salts of cobalt	Flesh-coloured precipitate.
Protosalts of iron	Salmon-coloured precipitate.
Persalts of iron	No change.
Caustic alkalies	Turn the red-coloured solutions of an orange colour.

The beautiful colour immediately produced on the addition of a soluble sulphide, is a most marked character of the nitroprussides. This purple coloration is most intense, and enables the detection of the most minute quantity of either reagent. As a test for the presence of sulphides it is wonderfully useful, enabling minute quantities of them to be found in circumstances where the ordinary means of testing altogether fails to denote their presence. This purple coloration is however only transitory, the compound soon breaking up into various substances, among which, hydrocyanic acid, ammonia, nitrogen, oxide of iron, a ferrocyanide, a sulphocyanide and a hyponitrite may be recognized.

The soluble nitroprussides are decomposed when sulphuretted hydrogen is passed through them, oxide of iron, prussian blue, sulphur, a ferrocyanide, and a peculiar sulphur compound being among the products of decomposition.

The alkalies decompose the soluble nitroprussides when their solutions are mixed together and boiled. The products of the transformation in this case are oxide of iron, nitrogen, a ferrocyanide and a hyponitrite. An excess of ammonia, even in the cold, gradually decomposes the nitroprussides, nitrogen gas being evolved, and a peculiar uncrystallizable black compound remains as the result of the decomposition.

Sulphurous acid, the sulphites and hyposulphites exert no apparent action on the nitroprussides. They are however wholly decomposed by boiling them with concentrated sulphuric acid; during this decomposition, the peculiar purple colour due to sulphides is observed.

Chlorine does not produce any change when passed through solutions of the nitroprussides.

Prussian blue dissolves in an excess of some of the nitroprussides, forming a beautiful blue solution; when the prussian blue is in excess, it is able, under certain circumstances (see § 5), to remove the soluble nitroprusside from solution, but it again yields it up to boiling though not to cold water.

Some of the nitroprussides are very permanent and suffer no change in solution, either by exposure to the air or by the

action of heat. Several, on the contrary, especially nitroprussic acid, the nitroprussides of barium, calcium and ammonium, decompose partially, either when their solutions are long kept, or speedily when they are boiled. Some of the products of decomposition are dissolved by the still undecomposed nitroprusside, and cannot be again separated from them by crystallization.

After this general idea of the habits of the nitroprussides, their individual salts and their transformations may be more easily studied.

Nitroprussic Acid.

10. This acid may be obtained in solution by decomposing nitroprusside of silver with an equivalent quantity of hydrochloric acid, or by precipitating nitroprusside of barium with an equivalent quantity of sulphuric acid. It may also be obtained, but in a less pure state, by precipitating nitroprusside of potassium dissolved in a small quantity of water, and diluted with several times its volume of alcohol, with an alcoholic solution of tartaric acid, the quantity of the latter being just sufficient to form bitartrate of potash with the potassium; but as the acid dissolves some of the latter salt, this process does not yield a pure product.

A dark red-coloured solution, strongly acid, is obtained by these methods. Æther does not precipitate the acid as it does ferrocyanic acid. Soon however the solution begins to form hydrocyanic acid, and either to deposit oxide of iron or to hold iron in solution, which may be detected by a prusside. When this change has taken place, evaporation *in vacuo* over sulphuric acid yields crystals of the acid, which is however found to contain a small quantity of an impurity, probably of a cyanide of iron, which cannot be separated by crystallization, or any other of the numerous methods tried. The amount of this impurity is from 2 to 3 per cent. This crystalline acid belongs to the oblique system, and its crystals are described and measured in a further part of this paper, together with its analyses. It possesses all the properties of nitroprussic acid, and only differs by containing this small quantity of impurity. The perfectly pure acid in crystals has not been obtained, notwithstanding very many efforts to obtain this desirable result.

Nitroprusside of Sodium.

11. This salt is the most readily procured, in a crystallized state, of all the nitroprussides; it may be obtained by decomposing the nitroprussides of copper or iron by means of soda,

filtering from the oxides of these metals and evaporating the solution by a gentle heat. When prepared from the iron salt, it is apt to contain a little iron in excess.

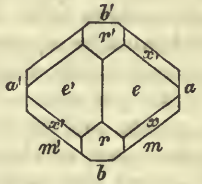
Nitroprusside of sodium is however most easily prepared in the following manner:—1 equiv. of yellow prusside of potassium is digested with 5 equivs. nitric acid, as described in page 202, until the solution precipitates salts of protoxide of iron of a slate colour. It is now neutralized with carbonate of soda, both solutions being employed cold. The neutralized liquid is now boiled, and the green precipitate is separated by filtration from the dark red-coloured solution. This is then evaporated down and again filtered from a brown precipitate which falls during evaporation. The nitrates of soda and potash are allowed to crystallize out.

The dark red solution is now evaporated on the sand-bath, and during evaporation prismatic crystals separate from the hot solution. These are removed, dissolved in water, and again crystallized by allowing the solution to cool. The reason of taking the crystals from the hot solution in the first instance is to obtain them uncontaminated with the nitrates, which are more soluble in hot water than this nitroprusside. By this process any quantity of the nitroprusside of sodium may be obtained in fine large ruby-coloured crystals.

Properties.—This salt crystallizes in fine ruby-coloured prisms, which have been measured by Prof. Miller.

Symbols:—*a* 100, *b* 010, *e* 101, *r* 011, *m* 110, *x* 211. *x* is common to the zones *em*, *ra*. The angles between the normals to the faces are,—

	°	'
ba	90	0
ea	68	16
ee'	43	28
rb	62	26
rr'	55	8
ma	52	38·5
mb	37	21·5



	°	'
mm'	74	43
rm	68	25
re	34	34
em	77	1
xm	49	24
en	27	37

Nitroprusside of sodium resembles very much in appearance the ordinary red prusside of potassium when the latter salt is crystallized from alkaline solutions*.

Nitroprusside of sodium is not at all deliquescent, but is

* Red prussiate of potash crystallizes more easily and with much greater beauty from alkaline than from neutral or acid solutions; the reason being that the excess of alkali decomposes a small quantity of a green precipitate, which crystallizes along with it.

very soluble in water, dissolving in $2\frac{1}{2}$ times its weight of water* at 60°. It is still more soluble in hot water, but appears to have a point of less solubility at a particular temperature, for it may easily be crystallized by keeping its hot solution on the sand-bath, while it may not do so on cooling.

It is decomposed by mixing it with excess of alkali, and suffers the singular transformations with sulphurets of the alkaline metals which have been already alluded to. It undergoes no change in weight when heated to 212°, and therefore does not lose water in the water-bath.

The following analyses were made by heating the salt with sulphuric acid, and estimating the iron as peroxide, the sodium as sulphate of soda:—

Analyses I. and II. were made upon a salt obtained by acting on nitroprusside of iron with caustic soda. III. and IV. from a salt prepared from nitroprusside of copper. V. VI. and VII. from the process last described, by acting on yellow prusside of potassium with nitric acid and neutralizing with carbonate of soda; and analyses VIII. and IX. from another preparation in the same way.

I. 11·80 grs. gave 3·30 grs. peroxide of iron and 5·87 grs. sulphate of soda.
II. 10·30 grs. gave 2·93 Fe^2O^3 and 5·00 NaO, SO^3.
III. 13·767 grs. gave 3·813 Fe^2O^3 and 6·44 NaO, SO^3.
IV. 21·536 grs. gave 5·932 Fe^2O^3 and 10·41 NaO, SO^3.
V. 19·610 grs. gave 5·47 Fe^2O^3 and 9·89 NaO, SO^3.
VI. 13·545 grs. gave 3·74 Fe^2O^3 and 6·45 NaO, SO^3.
VII. 15·740 grs. gave 4·42 peroxide of iron.
VIII. 13·788 grs. gave 3·88 Fe^2O^3 and 6·71 NaO, SO^3.
IX. 25·155 grs. gave 7·028 Fe^2O^3 and 12·12 NaO, SO^3.

The combustions were made with chromate of lead.

I. 9·188 grs. gave 6·870 grs. carbonic acid and 1·30 gr. water.
II. 8·580 grs. gave 6·315 CO^2 and 1·224 HO.
III. 13·815 grs. gave 10·08 CO^2 and 1·78 HO.
IV. 8·765 grs. gave 6·57 CO^2 and 1·28 HO.
V. 12·010 grs. gave 8·79 CO^2 and 1·45 HO.
VI. 15·070 grs. gave 10·79 CO^2 and 1·82 HO.
VII. 9·000 grs. gave 6·58 CO^2 and 1·11 HO.
VIII. 8·645 grs. gave 6·34 CO^2 and 1·184 HO.
IX. 10·921 grs. gave 8·035 CO^2 and 1·309 HO.

* 50·12 grs. saturated solution at 60° gave 14·46 salt; in another experiment 42·88 grs. solution gave 12·45 grs. salt, both being dried in the water-bath.

The nitrogen in this salt was determined by Dumas' quantitative method, an air-pump being used, so as to facilitate the expulsion of air from the apparatus.

I. 7·903 grs., by Dumas' quantitative method, gave 117 CC. gas; thermometer 11°·1 C.; barometer 30·415 inches. Hence the nitrogen is 27·781 per cent.

II. 4·6 grs., also treated by Dumas' method, gave 68 CC. gas; thermometer 45° Fahr.; barometer 30·742 inches. Percentage of nitrogen 28·79.

	From Iron Salt.		From Copper Salt.	
Iron	19·576	19·912	19·387	19·281
Sodium	16·114	15·718	15·160	15·795
Carbon	20·392	20·073	19·899	20·442
Hydrogen	1·572	1·585	1·437	1·622
Nitrogen	42·346	42·712	27·781	42·860
Oxygen			16·336	
	100·000	100·000	100·000	100·000

	From Prusside of Potassium.				
Iron	19·525	19·32	19·56	19·69	19·59
Sodium	16·348	...	15·88	15·90	15·76
Carbon	19·960	19·53	19·94	20·00	20·06
Hydrogen	1·340	1·34	1·37	1·52	1·33
Nitrogen	28·790	...	43·25	42·89	43·26
Oxygen	14·037				
	100·000	100·00	100·00	100·00	100·00

In order to estimate the water with more precision than can be done in an organic analysis, a portion of salt was heated in an F tube to which a chloride of calcium tube was attached: 9·52 grs. gave 1·20 gr. water, equal to 1·40 hydrogen per cent.

The above analyses correspond to the following calculated formula:—

			Calculation.
5	Iron	140	19·33
5	Sodium	116	16·02
24	Carbon	144	19·89
15	Nitrogen	210	29·00
10	Hydrogen	10	1·38
13	Oxygen	104	14·38
		724	100·00

It is obvious that if the analyses would authorise 25 equivs. of carbon instead of 24, a very much more simple formula might be given. The mean proportion of iron to carbon is

19·54 : 20·03, while the proportion, 5 equivs. : 25 equivs., or 1 : 5, would require 19·54 : 20·93 of carbon. Throughout all the salts, this less quantity of carbon refuses to enrol itself in the simple proportion of 1 : 5, and necessitates the use of the much more complex one of 5 : 24. The above formula may be expressed as $Fe^5Cy^{12}3NO, 5Na+10HO$.

Nitroprusside of Potassium.

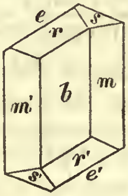
12. This salt may be obtained in several ways. 1. By acting upon prusside of potassium with nitric acid, exactly as described under nitroprusside of sodium, but the neutralization of the acid is effected by carbonate of potash, instead of carbonate of soda as therein described. The nitrate of potash is crystallized out and the mother-liquor is put in the hot chamber to crystallize. 2. It may be prepared from the nitroprusside of iron, or better from the copper salt, by decomposing it with caustic potash, care being taken to keep the nitroprusside in excess.

Properties.—This salt, from its great solubility, is somewhat difficult to crystallize. It is apt to deposit in an amorphous form; but this may be avoided by a little practice, and fine large crystals may be obtained. These crystals belong to the oblique system, and have been measured by Prof. Miller.

Symbols:—*b* 010, *m* 110, *s* 012, *e* $\bar{1}01$, *r* $\bar{1}11$.

Angles between normals to the faces:—

	°	′
ab	30	0
rb	54	5
mb	49	46
sb	68	52
em	113	55
es	57	7
sm	69	3



The axis of the zone *mb*, makes an angle of 57° 56′ with that of the zone *rb*, and an angle of 71° 0′ with the axis of the zone *sb*.

This salt dissolves in its own weight of water at 60°; 60·06 grs. of a saturated solution of this salt evaporated in the water-bath left 30·40 grs. of the salt. It is not precipitated from its solution by alcohol. With caustic potash it unites and forms a salt which is described in a further part of the paper. Nascent hydrogen does not decompose it. Hydrogen, chlorine and sulphurous acid were passed through both cold and hot solutions of the salt without effecting any change. It is slightly deliquescent, and acquires a greenish shade when exposed to

light; its solutions on long keeping deposit prussian blue and become partially decomposed.

The crystals of this salt are of a dark red colour.

The analysis was made by decomposing the salt by Nordhausen sulphuric acid. The following estimations give the amount of water lost in the water-bath:—

I. 14·865 grs. lost 1·765 gr., or 11·873 per cent.
II. 15·455 grs. lost 1·855 gr., or 12·002 per cent.
III. 12·430 grs. lost 1·480 gr., or 11·906 per cent.
IV. 20·155 grs. lost 2·245 grs., or 11·138 per cent.

Mean . . 11·730

The inorganic analyses yielded the following results:—

I. 23·905 grs. gave 6·479 grs. peroxide of iron and 13·837 grs. sulphate of potash.
II. 20·145 grs. gave 5·525 Fe^2O^3 and 12·105 KO, SO^3.
III. 13·015 grs. gave 3·550 Fe^2O^3 and 7·66 KO, SO^3.
IV. 12·945 grs. gave 3·536 Fe^2O^3 and 7·60 KO, SO^3.
V. 17·195 grs. gave 4·832 grs. peroxide of iron.

The organic analyses were made with chromate of lead.

I. 7·475 grs. gave 0·448 gr. water and 5·403 grs. carbonic acid.
II. 7·122 grs. gave 0·425 HO and 5·105 CO^2.

	I.	II.	III.	IV.	V.	Mean.
Iron . .	18·972	19·198	19·093	19·120	18·901	19·056
Potassium	25·947	26·934	26·385	26·388	...	26·413
Carbon .	19·712	19·548	...	...	...	19·630
Hydrogen	0·665	0·663	...	...	...	0·664
Nitrogen, Oxygen	34·704	33·657	...	...	...	34·237
	100·000	100·000				100·000

These results may be expressed by the following calculation:—

		Calculated.	Mean.
5 Iron	140	18·92	19·056
5 Potassium . .	195	26·35	26·413
24 Carbon . . .	144	19·46	19·630
3 Hydrogen . .	3	0·40	0·664
15 Nitrogen . .	210	28·38	34·237
6 Oxygen . . .	48	6·49	
	740	100·00	100·000

According to this calculation the formula of the salt dried at 212° is $F^5Cy^{12}3NO, 5K+3HO$; the salt loses in the water-bath 11·73 per cent. of water; had it lost 12·7 per cent. this

would have corresponded to 12 equivs.; 11 equivs. would yield a loss of 10·6 per cent.

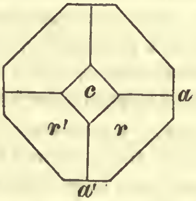
Nitroprusside of Barium.

13. This salt is obtained by decomposing nitroprusside of copper by caustic barytes, avoiding an excess of the latter. On filtration a dark red-coloured solution passes through. When evaporated under the air-pump, it forms fine large pyramidal crystals. The following measurements have been made by Prof. Miller of Cambridge :—

Symbols :—*a* 100, *c* 001, *r* 111.

Angles between normals to the faces :—

	°	′
ac	90	0
aa′	90	0
rc	44	35
rr′	59	30
ra	60	15



This salt, out of a strong solution, also frequently crystallizes in flattened prisms; no doubt as a different hydrate.

Nitroprusside of barium is of a dark red colour, is easily soluble in water, and is not deliquescent. It deposits a brown precipitate on boiling, resembling oxide of iron, but which, in the specimen examined, also contained barytes. The salt, after it has experienced this change, crystallizes in the same form, but with impurities which cannot be separated by filtration or crystallization. Analyses of this altered salt are given in a subsequent part of this paper.

The salt crystallized in the air-pump lost water in water-bath.

20·415 grs. lost at 212° 3·110 grs. water $=15{\cdot}233$ per cent.
24·455 grs. lost at 210° 3·648 grs. water $=14{\cdot}917$ per cent.

The analyses were made by acting upon the salt by sulphuric acid in the usual way.

I. 20·791 grs. gave 12·173 grs. sulphate of barytes and 4·180 grs. oxide of iron.

II. 17·24 grs. gave 10·198 grs. BaO, SO^3 and 3·48 Fe^2O^3.

The combustions were made with chromate of lead.

I. 8·539 grs. gave 1·208 gr. water and 4·665 grs. carbonic acid.

II. 10·068 grs. gave 1·132 HO and 5·580 CO^2.

	I.	II.			Calculated.
Iron . . .	14·073	14·129	5	140	14·05
Barium .	34·446	34·791	5	343	34·43
Carbon .	14·899	15·075	24	144	14·45
Hydrogen	1·571	1·249	15	15	1·50
Nitrogen .	35·011	34·756	15	210	35·57
Oxygen .			18	144	
	100·00	100·00		996	100·00

In the above analysis the proportion of carbon to the iron is higher than obtained with the other salts, but the error is usually on this side when chromate of lead, as in this instance, is used in the combustion. It will also be seen in a further part of the paper, that a carbonaceous impurity, probably an attached cyanide, not separable by crystallization, but removed when it is converted into a silver salt, is produced when a solution of this salt is kept for some time, and it is possible that a small portion may be present in the salt analysed. If we could be assured of the absence of all impurity, which it will be afterwards seen that it is difficult to believe from the variable composition of this salt, it is obvious that the above analyses might be much more simply expressed by the following calculation:—

		Calculated.
2 Iron	56	14·03
2 Barium . . .	137	34·33
10 Carbon . . .	60	15·03
6 Hydrogen . .	6	1·50
6 Nitrogen . .	84	35·11
7 Oxygen . . .	56	
	399	100·00

On the first formula the dried salt would be $Fe^5Cy^{12}3NO$, $5Ba+15HO$, on the second Fe^2Cy^5NO, Ba^2+6HO. The water lost in the water-bath would in the first case correspond to 20 equivs., in the latter case to 8 equivs.

Nitroprusside of Silver.

14. This salt may be prepared by adding nitrate of silver to any of the soluble nitroprussides.

The colour of the salt varies according to its state of preparation, from a fleshy white to a pale buff. When dry it has a flesh colour. It is insoluble in water, alcohol and nitric acid. Hydrochloric acid decomposes it with the formation of nitroprussic acid and chloride of silver. The caustic alkalies decompose it, as they do the soluble nitroprussides generally:

ammonia dissolves nitroprusside of silver, but it soon deposits white crystals, which are apt to be contaminated by oxide of iron. These white shining crystals are a compound of the salt with ammonia, and are quickly decomposed, even by water alone, but very readily by water acidulated with nitric acid. Ammonia is now found in solution and nitroprusside of silver remains. If ammonia and nitroprusside of silver be boiled together, total decomposition takes place.

The salt was decomposed by sulphuric acid, the silver estimated as a chloride and the iron as peroxide. Each salt analysed was prepared at different times.

I. 14·788 grs. gave 2·749 grs. oxide of iron and 9·925 grs. chloride of silver.
II. 22·838 grs. gave 4·220 Fe^2O^3 and 15·18 AgCl.
III. 16·675 grs. gave 3·115 Fe^2O^3 and 11·09 AgCl.
IV. 26·545 grs. gave 4·970 Fe^2O^3 and 17·78 AgCl.

The combustions were made in the usual way.

I. 8·350 grs. gave 0·252 gr. water and 4·045 grs. carbonic acid.
II. 8·385 grs. gave 0·234 HO and 4·150 CO^2.
III. 7·900 grs. gave 0·183 HO and 3·820 CO^2.
IV. 9·415 grs. gave 0·120 HO and 4·577 CO^2.

As this salt was well calculated to give correct knowledge with regard to the composition of the nitroprussides generally, the nitrogen was carefully determined by the three best processes, viz. those of Dumas, Liebig and Bunsen.

I. Quantitative estimation of nitrogen:—

6·808 grs. salt gave 69 C.C. nitrogen gas,

the thermometer being 7°·7 C. and the barometer 30·094 inches. This makes the nitrogen 19·299 per cent.

II. Liebig's method:—

Tubes.	Vol. mixed gases.	Vol. after absorption.	Vol. of carbonic acid.
1.	21·0	8·15	12·85
2.	18·4	7·3	11·1
3.	24·0	9·25	14·75
4.	20·15	7·45	12·70
5.	13·3	5·35	7·95
6.	26·20	9·2	17·0
	123·05	46·70	76·35

Hence the proportion of nitrogen to carbonic acid is as 1 : 163. This, calculated on 13·288, the mean quantity of carbon, gives 19·02 per cent.

Bunsen's method:—

	Obs. vol.	Barom. inches.	Therm.	Col. merc.
Vol. of mixed gases (moist)	110·8	757·7	16°·2 C.	217·0
Vol. after absorption (dry)	46·2	761·9	16·2	218·0

Corrected vol. of mixed gases	66·801
Corrected vol. of nitrogen	25·800
Vol. of carbonic acid	41·001

Hence the proportion of nitrogen to carbonic acid is as 1 : 1·589, which calculated on 13·288 carbon, gives 19·512 per cent.

	I.	II.	III.	IV.			Calculated.
Fe	13·012	12·934	13·076	13·106	5	140	13·011
Ag	50·546	50·000	49·925	50·040	5	540	50·185
C	18·211	13·508	13·177	13·257	24	144	13·382
H	0·330	0·310	0·250	0·140	2	2	0·185
N	19·299	19·020	19·512	23·457	15	210	19·516
O	3·602	4·228	4·060		5	40	3·721
	100·000	100·000	100·000	100·000		1076	100·000

With a quantity of hydrogen so small as that in the above analysis, it is difficult to obtain accordant results in an organic analysis. A portion of well-dried salt was therefore heated in an F tube, to which a tube filled with chloride of calcium was attached.

5·375 grs. gave 0·085 gr. water, equal to 0·175 H. per cent.
4·000 grs. gave 0·065 gr. water, equal to 0·180 H. per cent.

It is therefore quite certain that the silver salt dried at 212° still retains 1½ per cent. of water. It loses however this water at a higher heat and becomes anhydrous. The formula of the silver salt is therefore $Fe^5Cy^{12}3NO,Ag^5+2HO$.

Nitroprusside of Copper.

15. This salt is obtained by adding a solution of a copper salt to that of a nitroprusside. As it is insoluble in cold water, and almost entirely so in hot, it may be washed to any extent.

It is of a pale green colour, which changes to slate colour when exposed to light in the moist state. It is quite insoluble in alcohol. It is decomposed by the caustic alkalies, first passing into a dark brown basic nitroprusside, and then into oxide of copper and a soluble nitroprusside.

Nitroprusside of copper, dried in the hot chamber at about 100° Fahr., still lost weight in the water-bath.

45·60 grs. lost in water-bath 4·525, or 9·922 per cent.
25·12 grs. lost in water-bath 2·870, or 11·425 per cent.

The analysis of the dried salt was made by decomposing it with sulphuric acid, and estimating the two metals as oxides, after separating them by sulphuretted hydrogen.

I. 22·24 grs. gave 6·325 grs. oxide of copper and 6·515 peroxide of iron.

II. 21·00 grs. gave 6·018 CuO and 6·120 Fe^2O^3.

The combustions were made with chromate of lead and with oxide of copper.

I. 8·100 grs. gave 0·230 gr. water and 6·343 grs. carbonic acid.

II. 7·977 grs. gave 0·240 HO and 6·217 CO^2.

III. 9·887 grs. gave 0·330 HO and 7·694 CO^2.

IV. 11·507 grs. gave 0·320 HO and 8·936 CO^2.

The nitrogen was determined in three different ways.

I. Dumas' quantitative method:—

6·226 grs. gave 98 CC. nitrogen gas. Barom. 30·105 inches. Therm. 8°·8 C.

II. Bunsen's method:—

	Vol.	Barom. inches.	Therm. °	Col. merc.
Vol. mixed gases (moist) .	246·3	29·988	15·6	219·7
Vol. after absorption (dry)	121·1	30·069	15·4	348·0

Corrected vol. of mixed gases . .	123·180
Corrected vol. of nitrogen	47·491
Corrected vol. of carbonic acid . .	75·689

Hence the proportion of nitrogen to carbonic acid is 1 : 1·593, which calculated on the mean quantity of carbon (21·25), yields 31·12 per cent. nitrogen.

III. Liebig's method:—

Tubes.	Vol. mixed gases.	Vol. after absorption.	Vol. of carbonic acid.
1.	21·2	8·0	13·2
2.	22·4	9·1	13·3
3.	26·0	10·4	15·6
4.	21·7	8·2	13·5
5.	28·3	10·6	17·7
6.	17·9	6·7	11·2
7.	22·2	8·2	14·0
8.	19·8	7·5	12·3
9.	20·0	8·0	12·0
10.	22·7	9·0	13·7
11.	28·0	10·8	17·2
12.	19·2	7·3	11·9
13.	14·6	5·4	9·2
	284·0	109·2	174·8

Hence the proportion of nitrogen to carbonic acid is 1 : 1·60.

	I.	II.	III.	IV.			Calculated.
Fe	20·506	20·400	...	...	5	140	20·43
Cu	22·708	22·880	...	...	5	158	23·06
C	21·351	21·255	21·222	21·179	24	144	21·02
H	0·315	0·309	0·371	0·308	1	1	0·14
N	29·856	31·120	30·980	...	15	210	30·65
O	5·264	4·036	...	...	4	32	4·70
	100·000	100·000				685	100·00

The formula of the copper salt is therefore $Fe^5Cy^{12}3NO, Cu^5+HO$.

Nitroprusside of Iron.

16. This salt is obtained by adding sulphate of the protoxide of iron to a soluble nitroprusside. When the solutions are dilute the precipitate does not at first appear; as however it is very sparingly soluble, it may be purified by washing either with hot or cold water.

This salt is a salmon-coloured precipitate, nearly though not absolutely insoluble in water; it is more soluble in water rendered acid by nitric acid. It is decomposed by caustic alkalies, with the precipitation of oxide of iron and the formation of a soluble nitroprusside. Before, however, being completely decomposed, a dark-coloured basic nitroprusside of iron is produced.

A salt dried in the hot chamber, at a temperature about 90° Fahr., still lost water when exposed in the water bath:—

14·162 grs. lost at 212° 2·890 grs., or 20·406 per cent.
10·893 grs. lost at 212° 2·320 grs., or 21·298 per cent.
17·500 grs. lost at 212° 3·545 grs., or 20·257 per cent.

In the two first analyses given below, the iron was determined by decomposing the salt by sulphuric acid, oxidizing with nitric acid and precipitation by ammonia. The third estimation was by calcination, a little nitrate of ammonia being used to effect complete oxidation.

I. 18·075 grs. gave 9·917 grs. peroxide of iron.
II. 30·935 grs. gave 16·900 grs. peroxide of iron.
III. 9·220 grs. gave 4·995 grs. peroxide of iron.

The combustions were performed with chromate of lead.

I. 7·218 grs. gave 0·717 gr. water and 5·255 carbonic acid.
II. 7·347 grs. gave 0·810 gr. water and 5·360 carbonic acid.
III. 6·360 grs. gave 0·693 gr. water and 4·695 carbonic acid.

The nitrogen was determined by Dumas' quantitative method.

5·427 grs. gave 86 CC. nitrogen gas, the thermometer being 48°·7 Fahr. (9·4 Cent.) and the barometer 29·285 inches.

	I.	II.	III.			Calculated.
Iron . . .	38·406	38·241	37·922	10	280	38·35
Carbon .	19·855	19·896	20·136	24	144	19·72
Nitrogen	29·285	29·285	29·285	15	210	28·76
Hydrogen	1·103	1·224	1·210	8	8	1·09
Oxygen .	11·351	11·354	11·447	11	88	12·08
	100·000	100·000	100·000		730	100·00

The formula of the iron salt, dried at 212°, would therefore be

$$Fe^5\,Cy^{12}\,3NO\,Fe^5+8HO.$$

Nitroprusside of Zinc.

17. This salt is prepared by precipitating one of the soluble salts of zinc by a nitro-prusside. It is a salmon-coloured precipitate, of a more fleshy colour than the iron salt. When formed slowly, as when muriatic acid and zinc are made to act on nitroprusside of soda, it is of a deep orange colour.

Nitroprusside of zinc is very slightly soluble in cold water, rather more so in hot water. In its behaviour to reagents it acts exactly like the iron nitroprusside. It was analysed by decomposing it with sulphuric acid, separating the iron by succinate of ammonia and determining the zinc as a carbonate.

I. 24·14 grs. gave 6·92 grs. peroxide of iron and 670 grs. oxide zinc.

9·43 grs. gave 7·10 grs. carbonic acid and 0·335 gr. water.

				Calculated.
Iron	20·07	5	140	20·11
Zinc	22·26	5	160	22·98
Carbon . . .	20·53	24	144	20·69
Hydrogen .	0·39	2	2	0·28
Nitrogen } Oxygen }	36·75	15 5	210 40	35·94
	100·00		696	100·00

This analysis would lead to the formula

$$Fe^5\,Cy^{12}\,3NO\,ZN^5+2HO.$$

[To be continued.]

XXVIII. *On the Meteor which appeared on Monday, the* 11*th of February* 1850, *at about* 10h 45m P.M. *By* James Glaisher, *Esq., F.R.S.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

The magnificent meteor which was seen all over England on the night of February 11, 1850, was so remarkable in many respects, that I beg to send you the following particulars of it.

In consequence of the deception of the senses on the sudden appearance of unusual objects, the short time of continuance of this meteor, and the fact that in many cases the observations were made by gentlemen unaccustomed to think upon these subjects, I think it necessary to give all the accounts I have received.

As yet I have not heard that this body was seen south of England, and the following are the accounts from different places arranged in the order of latitude.

I. From Wootton, three miles from the south coast, and situated between Lymington and Christ Church. Thomas Falconer, Esq., says, "I had just retired to bed when the room was first illuminated with a pale light, and instantly afterwards everything in it was visible; nor can I describe the brilliancy of the light more than by saying, that a deaf and dumb brother, who has been taught to speak many words, rose from his bed and gave an alarm of fire. I was not able to be quick enough to get to a window from which I might have seen it, but there was most certainly not the slightest noise."

II. From Brighton. The Rev. J. Wharton favoured me with the following:—

"When the meteor exploded, it appeared to be about five degrees west of north, and at an altitude of about 5° to 7°* above the horizon, as I observed it at this place, top of Brunswick Square, Brighton. The light appeared of a very pale bluish description and exceedingly brilliant; but I only caught it at the moment of explosion, because it was previously behind a layer of clouds."

III. From Southampton, I received the following from W. Philip, Esq.:—

"About fifteen minutes to eleven on Monday night, as I was walking through the High Street due north, I saw a large, as it appeared to me, ball of fire somewhat exceeding the dimensions of a full-sized orange, and intensely bright towards its edge, pass with great velocity from west to east, giving forth a light equal to that of a vivid flash of lightning interrupted occasionally by a few clouds, beyond which it passed and instantly disappeared. Its course was indicated by a bright luminous appearance like a tail; besides which it also threw off at right angles, as if by combustion, a number of white sparks in appearance like stars. I suppose I saw it altogether about two seconds, but did not hear any report when it dis-

* It is likely that the view from this gentleman's house does not meet with an unobstructed horizon, and that in reality the meteor was more elevated than 7°.

appeared, though I listened, and was at the time favoured by perfect stillness around. Although I mention that it was larger than an orange, I do not intend that it was perfectly round, as the edge appeared to be very jagged and irregular. Some friends in the town who had retired to rest were also alarmed by an unusual flood of light, which, at the same time as I have noted, illuminated their bed-chambers."

IV. From Yeovil, Somersetshire. J. Hannane, Esq., favoured me with the following information :—

"I saw on Monday, the eleventh instant, about half-past ten, a brilliant meteor; it first appeared of a red colour like Mars, but twice or three times its apparent magnitude; a little to the west of north, at an angle as far as I can judge of about 60 degrees, rather more than less, its course was easterly, forming a slight curve; the tail and meteor increased in brilliancy until I lost sight of it, owing to some houses intervening; but the light, after I had lost the view of the meteor itself, increased to that of the moon in a clear night when half-full.

From a red it assumed a brilliant white light; and I think it was not in one compact light, but rather like several lights together, as I was losing sight of it. The time that I first saw it, *for I saw its commencement*, until the reflexion of it was gone, may be about seven or eight seconds; the sky was not at all clouded in that direction. The day had been remarkably wet and stormy, but it had cleared about nine o'clock or a little before."

V. From Uckfield. C. L. Prince, Esq. favoured me with a letter containing the following particulars :—

"A very large meteor passed over this place on Monday night (11th inst.), and at a very low elevation, so that I cannot give you any idea of its path, further than its direction was from N.W. to S.E., and it at last burst at an apparent distance of four miles, making a rumbling noise like distant thunder, and which was distinctly heard by persons indoors*. The light was most intense, and I could have read a small print very distinctly. It was of bluish colour, and the time of its appearance was about $10^h\ 30^m$ to $10^h\ 40^m$ P.M. I cannot say exactly. It was seen by many persons in this locality, and some of the country people were much frightened by it. There had been much rain (·51 inch) during the afternoon, and the night was densely overcast and dark."

VI. From Langport, Somersetshire, and for which I am obliged to Wm. Bond Paul, Esq.

"I was fortunate to witness the splendid meteor which

* This sound must have been a deception.

occurred on the night of Monday the 11th instant, and I beg to send you the following account, although an incomplete one:—

The time of its appearance was at about twenty minutes to eleven P.M., London time; its course was from S.W. to N.E.

Its colour was pale blue, but very brilliant.

Its apparent magnitude was that of Venus when brightest; a lady who was with me thought it much larger.

Its duration was about three seconds.

It proceeded very slowly, and a pale blue train was visible behind it for many yards; after continuing its course for some distance an explosion appeared to take place, upon which it was immediately followed by three other globes in the same direction; they were visible however for a very short time only, and all disappeared simultaneously.

I considered its path to be horizontal.

Its distance from me was seemingly inconsiderable, and an observer might easily have imagined the meteors to have been within a few hundred yards of him.

Surrounding objects were rendered distinctly discernible."

VII. Bath. The following was copied from the Bath Chronicle of February 16.

Meteor.—On Monday night last, shortly after half-past ten o'clock, a meteor of great magnitude and brightness descended in a direction nearly eastward of this city: on its first appearance it lighted up the whole visible horizon with an intense glare equal to that of the most vivid and sustained lightning; as it proceeded in its course, the colour of its light changed from red to intense blue, and its explosion was unaccompanied by any noise, but it appeared to dissipate in a shower of sparks. Its apparent size was that of a globe, either eight or nine inches in diameter, and it left a stream of fire in its course, occupying some 25 degrees of the heavens. The weather was clear and calm at the time, but the day had been squally with drenching rain.

VIII. Royal Observatory, Greenwich. The following observations were made by the Astronomer Royal :—

On the night of Monday, February 11, I was standing in the Computing Room, at a distance of about 4 feet 6 inches from the western wall of the room, and 3 feet 4 inches from the pier between the central and western windows on the north side of the room, with my eyes glancing downwards to some papers on the table (I think my lamp was in the window-seat of the west window), when my attention was attracted to a general light of strong yellow colour upon that part of the sky which was seen through the western window.

I raised my eyes and saw the sky most fully illuminated; the form of the north-east dome and the walls near it (which had been invisible before and were totally invisible afterwards, from the room, while my lamp was in it) were brought out not only distinctly but conspicuously, I think as well as if a large moon had been behind the dome, but the colour of the light was so different as to make comparison difficult. In an instant there came in the direction from W.S.W. to E.N.E. a brilliant body like a congreve rocket, followed by two others close behind it (I cannot assert that there were not more than two) in the same path. The direction of their path was nearly horizontal, but slightly rising, I think. The height above the top of the dome was nearly equal to the semidiameter of the dome, I think rather less.

I think that the meteor passed my field at the western window in less than $1^{s}\cdot5$; and I saw nothing of it at the central window. All was very thickly dark as before. My first impression was that it was a rocket; on a moment's consideration I saw that it was impossible that a rocket could have such a nearly horizontal course except fired from the leads of my own house. I then looked at my watch, and, allowing as well as I could for the few seconds past, the watch time was $10^{h}\ 42^{m}\ 10^{s}$. By a comparison with the ball clock made immediately afterwards, the watch was 42^{s} fast on Greenwich mean solar time.

On going into the open air, I found that that part of the sky, and the north generally to the height of 30° or 40°, was starlight, and that there were stars visible over head, but the south was clouded.

From careful measurements afterwards, and calculation, its elevation at this part of its path was nearly 14° 53′, and its azimuth 19° west of north.

IX. Euston Square, London. George F. Burder, Esq. favoured me with a letter containing the following particulars:—

"I was standing at a window facing E.N.E. in the night in Euston Square with the window open, observing an auroral light over the north horizon, when my attention was attracted by a sudden and brilliant illumination of the whole sky and of terrestrial objects. I cast my eyes in the direction from which its light appeared to proceed, and presently there came into view in N.N.W. (having been previously concealed by houses) a meteor of almost dazzling brightness, which, after traversing a certain further distance in the sky, instantaneously vanished. The point of its disappearance was about north or east of north, but certainly not so far east as

N.N.E. Its course appeared to me (in the part I saw) to be nearly parallel to the horizon, or rather inclining towards it; and its altitude I guessed roughly at twenty degrees. The appearance of the tail was totally unlike either of the illustrations in the Illustrated London News,—it had rather the appearance of a compact sheet of flame thrown behind by the rapidity of the motion, commencing with the diameter of the head and tapering to a point at the extremity:—in fact just such as would be produced by throwing an ignited tow-ball into the air. I saw no fragments or sparks thrown off in its course, or anything like bursting at its disappearance; it seemed to me simply to go out, and that instantaneously. I heard no explosion or noise of any kind. The time of its occurrence I made about 10^{h} 41^{m} 30^{s} P.M. Greenwich time, and I can speak pretty confidently within about half a minute, but not nearer. The whole duration of the phænomenon was probably between 2 and 3 seconds; the part of its course which I actually saw (from N.N.W. to about north by east) may have occupied half that time. The whole of the sky visible from the window at which I stood was free from cloud, except a few slight streaks of haze radiating from a point in N.E. I am not sure that these may not have been auroral streaks. The appearance over the north horizon was clearly auroral and not haze, for α Lyræ shone brightly through the thickest of it."

At my request Mr. Burder took the trouble to measure the height of its apparent elevation, as well as he could call it to mind. The following is a copy of his second letter:—

"Having at hand a quadrant which I am in the habit of using, attached to a small telescope, I thought it unnecessary to put in practice the suggestions you offer for ascertaining the altitude of the meteor. I therefore adopted the following plan for recalling, as accurately as possible, the original impression. This evening, the sky being clear, I stationed myself in precisely the same position that I occupied when I saw the meteor, and at the same hour, and endeavoured to trace, as near as possible, from recollection its course in the heavens. In this I was assisted by the stars, having observed at the time, in a general way, its position in relation to the principal stars in the neighbourhood. I then fixed upon a star which seemed to the best of my judgement to correspond in altitude with the point at which the meteor disappeared, and directing the telescope thereto, found the altitude to be about 23°.

"I think therefore if I say that it disappeared at that altitude and at a point in the azimuth about 10° east of north, I shall not be very far wrong."

X. From Regent's Park, as observed by J. R. Hind, Esq.

"I saw the meteor for a moment, but being unfavourably placed at the time, I could only see that it moved from below α Persei towards α Cassiopeia, near which star it must have been when I lost sight of it. The appearance of its light was such, that in my idea no doubt can be entertained but that it was of electrical origin; it moved precisely in the direction in which the wind was blowing at the time. About us, the houses, &c. were as well seen as by daylight, when the sun is shining brightly. The meteor occurred at $10^h\ 39^m$ mean time."

XI. From Enstone, Oxon. The Rev. J. Jordan, the Vicar, in a letter to the Astronomer Royal, says,—

"That he had been in bed about ten minutes, when, it being then about a quarter before eleven, a flash of light appeared upon the window, increasing in intensity as it continued; its permanence of several seconds, proved it not to be lightning: some time after the light had disappeared, a sound was heard as of a dreadful explosion, which shook the window of the room and the whole of the house. The sound was utterly unlike thunder, even when taking effect upon any object.

"The next morning, the noise of the explosion, as well as the light which had been seen, were in every one's mouth; several thinking, as I had done, that some persons had come under their windows with a dark lanthorn. My own servants were greatly alarmed, and believed that a violent entry had been made into the house, mistaking the explosion for the falling of a heavy shutter.

"Upon further inquiry, reports were given of an extraordinary ball of fire having been seen in the air; and learning that one young man had seen it under peculiar advantageous circumstances, I went with him to the spot where he had seen it, and had from his mouth the following details:—He had gone out with a lanthorn to cut some hay from a rick, and having set the lanthorn on the ground, was in the act of using the knife and cutting the hay, when he found himself in a blaze of light. His first fear was that from negligence he had set the rick on fire, and he turned to look for his lanthorn, when as he turned, his eye was caught by a large ball of fire of a bluish light in the sky. It was moving steadily along when it burst out with sparks of fire, and continued to do so until it went down. It moved along directly in front of him, so that he could distinctly point out to me its path, which was about a third in height from the horizon to the zenith, in direction nearly due north and south, and in length full one-third of that part of the circle of the sky along which it was moving. Having thus seen it, he was left for the instant, so

great was the change, in total darkness; but turning to his knife in the hay, he finished cutting what he wanted, took it up with his lanthorn, went round two sides of the rick (a small one), opened a door into a loft, threw the hay up into the loft, shut the door and fastened it, and then went round the stable to a gate into the stable-yard; as he reached this gate he heard the explosion which everyone else had heard, and calculated that it must have been at least a minute and a half, perhaps two minutes, from the time the light disappeared to his hearing the sound. I judged the time between my losing sight of the light and hearing the sound to have been the same, and as also do many others, who both saw the light and heard the sound.

"The sound was heard distinctly and loudly everywhere, and in every house around here. Some hearing it, fancied that barrels had burst in their cellars, and got up and went down to see; others fancied that a door or shutter was broken open and thrown down, and also got up to see; the alarm was general, so loud was the noise, and so peculiar the sound as differing from that of thunder. One young man sat reading after being left by a few friends who had been practising music, and a large drum that stood near him with the sticks upon the head of it, was so affected by the sound that the drumsticks were slightly raised by its reverberation."

XII. Hampstead Road. From the Illustrated London News of Feb. 16, 1850.

"I was an eye-witness of a splendid meteor on the night of Monday the 11th instant, at twenty minutes to eleven o'clock. It first appeared like a star, about four times as large as Venus, with a dull golden lustre, and rapidly increased in brightness till it became a white light, resembling an immense diamond, and put forth a tail like a waving blade of red flame; which, as it proceeded, either disappeared or was lost in the increased brightness of the head, which at last shone so brilliantly as to light up the whole atmosphere. The light certainly far exceeded that of the full moon; it then became suddenly extinguished; it evidently displayed the process of combustion. I first saw it somewhere a little above the Pleïades, and it descended obliquely towards the north, and disappeared above the lower part of Cassiopœïa. I think its greatest length was about 2°; the length of its visible path about 15° or 20°; and the time it was visible three or four seconds. I thought the last I saw of the meteor as the light vanished was a red spot, but it disappeared instantaneously. I looked round at the clock directly it had disappeared, and it was exactly twenty minutes to eleven.

Feb. 13, 1850. G. Bowles, Jun."

XIII. From Hampstead, as taken from the Illustrated London News of Feb. 16, 1850.

"Last evening I saw a most extraordinary meteor, of which I send you a sketch* as it appeared over Hampstead. A straight black cloud extended from south to north, and the stars on either side of this cloud were unusually brilliant. When the phænomenon alluded to occurred, it issued from about S.W., and appeared to travel slowly in a direction about N.N.E. On its first emerging from its hiding-place it was a clear white light; about midway of its flight it assumed the brilliant yellow of the light produced in the combustion of sodium; and before it departed its colour was of that peculiar pink colour of potassium when burning.

Feb 11, 1850. ROBERT LONGBOTTOM."

XIV. From Brompton Hospital of Consumption, as extracted from the Illustrated London News of Feb. 16, 1850.

"The brilliant meteor which passed over the metropolis on Monday night, had a singular effect on the numerous patients in the several wards of this hospital. It appeared to rise immediately at the back of the new chapel now erecting in connexion with the institution, and rushing over the hospital, diffused a light of the most fearful intensity. To those only slightly acquainted with meteorological phænomena, the sight was alarming; but to the naturally timid the effect was most distressing. The shock was felt so severely by the matron of the establishment, that a severe fit of illness supervened, from which it is to be regretted that on Thursday she had scarcely recovered."

XV. Dr. Lee, at Hartwell, says that the meteor was not seen there, but that its light was, and the report was heard. Mr. Samuel Horton at Hartwell says that he heard that William Miller, a watchman at Dinton, which is situated at about two miles from Hartwell, saw the meteor, and from him he learnt the following particulars:—"That about a quarter to eleven o'clock on Monday evening, February 11, he observed something like a great mass of fire in the heavens in the west, having a long tail; and the mass was as large as the full moon, but giving a great deal more light; it passed along the sky from west to east, gradually decreasing in height in its progress. It lasted, he thinks, about two or three minutes before it exploded, and then it had the appearance of a rocket; when it burst, a report like thunder followed about two or three minutes after the light was gone; and he says that he

* The sketch is engraved in the paper.

thinks the thunder came from the north; there was no cloud to be seen at the time, and the air was very calm."

XVI. From Bromham, near Bedford. The Rev. J. S. Goodall, the Vicar, favoured me with a letter containing the following particulars:—

"Not only was the meteor of the most startling brilliancy, but the roar of the detonation was heard and proved to be of a most peculiar description—not like that of thunder above the earth, but as it were upon the ground or even under it; thus of those of whom I have had any communication upon the subject, some say it was quite like a luggage-train emerging from a tunnel; others like heavy fire-engines going full-speed along a road pitched with pebbles. To all persons the vibratory noise seemed close at hand; and persons have told me of their dashing from the centre of the road thinking they should be run over, and others who were at home were fully persuaded that fire-engines had driven into their yard. One servant of a friend of mine cried outright. In my own house alarm possessed us in various ways; I myself have the misfortune to be very deaf, and rarely hear thunder, though alive to the vibratory motion which accompanies it. On the night of the 11th of July I was writing in my study, when on a sudden I felt the table and floor shake very perceptibly, at the same moment my dogs came running in and out yelling most piteously. On going upstairs I found all in dismay, not so much at the glare of light (that was said to be unequal to twenty moon lamps in the room, and continuing for full half a minute) as from the dreadful crash and rumbling, which they all said was not thunder: from the red colour of the globular ball and the silver streams of light which followed it, I suppose there can be little doubt of its being one of the class of Aërolite."

XVII. From Rugby, the account I received was more particular, and for which I am obliged to the Rev. H. Highton, M.A.

"1. Time. The school clock struck $10^{h}\ 45^{m}$ immediately after the explosion was heard. I suspect it was a few minutes fast by Greenwich time.

"2. The light quite obscured the gas-light. Some say they could have read the smallest print by it. Some compare it to a strong sun-light. All speak of its similarity to the electric light in colour and vividness.

"3. Direction. From W. to E., or W.N.W. to E.S.E. through the zenith. It exploded so soon after reaching the zenith, that the fragments are described as appearing to fall a few yards off.

"4. Duration of the light, about twenty seconds.

"5. The explosion was like that of a rocket; and fragments are described as being seen falling, though *not luminous.*

"6. The detonation was heard from seventy to ninety seconds after the explosion—I think not less than ninety. The noise was like that of the crash of a falling building, or, as one person describes it, as though a quantity of horses had broken loose and were galloping about.

"The duration of time is estimated by the spaces walked over by persons walking, between the appearance of the light, its explosion, and the sound being heard."

XVIII. From Birmingham. Edward Wheeler, Esq. favoured me with the following particulars:—

"I beg to offer you a description of the beautiful meteor which I saw here on Monday night, Feb. 11.

"In walking directly west, at twenty minutes to eleven, along Great Hampton Street (which is sixty feet wide), the road became so illuminated as to allow a pin to be seen.

"Thinking it was occasioned through the aurora borealis, I instantly turned towards the north, and saw a colour the most brilliant (about two feet wide and twenty feet long) falling rapidly from the zenith towards the south, displaying the gorgeous colours of the iris, and throwing out towards the head some pink and light blue fragments, which became altogether extinguished behind the buildings opposite to me, of which a sketch is now made for you.

"Just afterwards, in meeting a man and his wife, they seemed to be much alarmed."

XIX. Near Wolverhampton. From the Illustrated London News of Feb. 16, 1850.

"Allow me to briefly notice the appearance last night (Feb. 11) of a peculiarly large and brilliant meteor. After a stormy day of wind and rain, the latter ceased at sunset, but the former at the same time increased for a short space, and then gradually lulled. The night became calm and clear, with a few clouds on the horizon, and the stars shone with remarkable brightness. At a little before eleven o'clock I was struck by the sudden appearance of a brilliant light resembling a continued gleam of lightning, but which, on looking up, was found to proceed from an elongated luminous ball, falling rapidly from the zenith towards the eastern horizon. It appeared like a mass of molten metal, but little smaller than the moon's disc, and comparatively at a short distance from my place of observation. The light given off was intense, and rendered the whole landscape distinctly visible. When approaching the earth it seemed to burst,

but without noise. A shower of luminous fragments, like red-hot stones, was discharged, or rather fell through, but were soon extinguished. The whole phænomenon was visible, as well as I could judge, for about sixty seconds. In general appearance it more resembled what is usually understood by a meteor, but its magnitude and apparent nearness were remarkable. Had it however exploded with detonation, I should have supposed it to be an aërolite."

XX. From Carrington, near Nottingham. J. K. Sewell, Esq., in a letter to the Astronomer Royal, says—

"At about 45 minutes past 10 o'clock on the night of Monday the 11th inst., when descending the hill into Nottingham, from the north my attention was suddenly attracted by a bright light, apparently equal to that of a full moon in a clear sky, which brilliantly illuminated every object around. On raising my eyes to ascertain the cause of so unusual an appearance, I beheld a most splendid meteor, apparently very near and crossing my path in a direction from about S.W. to N.E. It was descending. Its colour was that of the flame of potassium on the surface of water, and of intense brilliancy. Its length was about 2° 30′. Its head was whiter than its central, which looked like a brilliant pink flame. It was visible to me during three or four seconds, and just before it disappeared seemed to be nearly poised in the air, but near to the earth, and emitted numerous red sparks from the end of its tail. No sound was audible."

At the request of the Astronomer Royal, Mr. Sewell went again to the spot where he was when he saw the meteor, taking with him a quadrant to measure such altitudes as he could best recollect, and a compass to take the bearing, and the following is the additional information furnished.

"Placing myself on the spot from which I saw the meteor, I found that the direction of my path exactly corresponded with that of the magnetic meridian*, consequently differing from that of the terrestrial meridian by the amount of the variation of the compass. On raising the quadrant as nearly as possible to the position in which the meteor first became visible to me, I found that its elevation must have been 13°; this agrees with the opinion of a friend with whom I was when the meteor was seen, and who kindly assisted me in endeavouring to fix its position."

XXI. Blakeney, Norfolk, as extracted from the Illustrated London News of Feb. 16, 1850.

"As I was driving home last night at twenty minutes to

* The variation of the compass was 23° 20′ nearly.

eleven, I saw a very large meteor under Orion's Belt, and midway between that and the horizon. Its appearance was instantaneous, and lasted about thirty seconds, emitting a most brilliant light, which had at the period of its great intensity a bluish tinge. So intense was the light, that I was enabled to see the very smallest objects with perfect distinctness. The course of the meteor was from north to south*, and it appeared to be not less than twenty feet in length, and contracted and expanded with the greatest rapidity, giving me the idea of an enormous flaming umbrella, which was open and shut alternately. The quarter of the sky in which it appeared was covered with black clouds."

XXII. My next information was from Hull, the following particulars being furnished by William Lawton, Esq.

"The meteor appeared at 20 minutes to eleven o'clock; and when I first saw it (which was not at the moment of its first appearance), it was 5° N.E. of Sirius, advancing to the E.S.E. in the direction of the stars ξ and 15 Argûs, near which it exploded at an altitude of 12°, and at about 15° west of the meridian.

"Its light was intense and dazzling in the extreme, and created in my mind the impression of being a body in course of explosion throughout its whole course; its size seemed to be about half the diameter of the moon.

"I noted the time and its path immediately after its disappearance, and with all the care the circumstances would permit, with the view of enabling myself and others to ascertain the length of its path and distance from the earth at the time of bursting.

"I heard no report or noise of any kind. There was a slight tinge of blue in its light, which far exceeded that of the full moon."

The position of the meteor at the time of explosion, being near 15 Argus, is well-marked; at the time of observation the star was near the meridian, and was 11° high at Hull; therefore 12°, as stated above, must be very near the truth.

XXIII. I shall conclude with one account more, which I have received from John Douglas, Esq., of Raby Castle Gardens, Standrop, Durham.

"At about fifteen minutes to eleven, at Raby, I was approaching the castle from S.E., when suddenly the whole surrounding atmosphere appeared brilliantly illuminated; my first impulse was hastily to survey the castle, thinking that

* This traveller, I think, must have been deceived in the direction of the meteor, probably by the windings of the road.

some part of the building might have taken fire; this occupied a second or two of time. Guided by a similar impulse, I wheeled round, and luckily I was just in time to observe what appeared to be an exploding ball of fire, similar in appearance to the representation in the London Newspaper, of the meteor as seen from the Fulham Road. To me it appeared to be falling in an easterly direction, at an angle of about 25° to the horizon. The meteor seemed to be suddenly extinguished in a clump of trees a little distance off, and to the S.E. of where I stood: as near as I can judge, the illumination lasted about 6″. The whole surrounding landscape was rendered distinctly visible. One of the Duke's huntsmen, who was about 100 paces from me when the meteor first appeared, and who ran up to me in great agitation, said that he could see my person as distinctly as if it had been noonday."

These are all the accounts I have seen upon this body, which must have been of considerable magnitude to have yielded light sufficient to illuminate every place situated within the distance of 200 miles of it. I hope yet to receive more information, as its course is by no means clear. The observer at Yeovil says that he saw its commencement, at an altitude of 60° fully, but he has omitted to state the direction in which he was looking. At Bath it is stated to have been eastward of the city; if both these accounts be correct, the meteor, if visible so soon, must have been vertical over parts of Wiltshire and Gloucestershire, but this does not seem to have been the case from the accounts from other places; I shall, therefore, not proceed to inquire into its true path, its figure, or its magnitude at present, but confine myself to an investigation of its distance from the earth at the time of its explosion.

The best data we have for this determination are from the observations of the Astronomer Royal at Greenwich, of the Rev. H. Highton at Rugby, and of William Lawton, Esq. at Hull. Assuming that the chord distance from Greenwich to Rugby is 86 miles, and that from Rugby to Hull is 105 miles, and that the radius of the earth is 20,970,260 feet; that the altitude of the meteor at Greenwich was 14° 53′ above the north horizon, and that at Hull was 12° above the south horizon, and that the meteor was vertical at Rugby,—the explosion took place at the distance of 23·2 miles from the earth, from the observations at Greenwich and Rugby, and at the distance of 23·3 miles from those of Hull and Rugby.

After a careful review of all the particulars, I shall give some additional results in another paper.

XXIX. Professor Powell and Dr. Whewell.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In a paper lately published by me among the Memoirs of the Oxford Ashmolean Society entitled "On Necessary and Contingent Truth," I have entered somewhat on the questions which have been so much discussed as to the origin of our ideas and foundation of our reasonings in mathematical and mechanical science; and in so doing have, of course, adverted, as their importance justly demands, to the speculations of Dr. Whewell on these points. I find, however, by a letter which I have since received from that eminent writer, that in the reference I make throughout my paper to his opinions I have unfortunately mistaken them, in ascribing to him the belief in *innate, inherent, or intuitive ideas*; or (as at p. 6) in "ideas dependent upon intuitive conviction." Such belief, Dr. Whewell, in his letter to me, distinctly disclaims. He has spoken of certain "fundamental ideas" appropriate to different branches of science, but has nowhere discussed the question of their origin; contending only that they are results of the active, not of the passive constitution of the mind.

I am anxious therefore to take the opportunity afforded by your Journal to express my regret at having been led into a misconception of Dr. Whewell's meaning; while I rejoice to find that, by the explanation thus given, the difference between the views which I have upheld and those of Dr. Whewell is materially diminished.

I remain,

Very truly yours,

Baden Powell.

XXX. *Proceedings of Learned Societies.*

CAMBRIDGE PHILOSOPHICAL SOCIETY.

[Continued from vol. xxxv. p. 392.]

Nov. 26, 1849.—On the Dynamical Theory of Diffraction. By Professor Stokes.

The problem of diffraction is treated mathematically by conceiving each wave of a series incident on a small aperture, or passing the edge of a diffracting body, broken up on arriving at the aperture or diffracting edge, regarding each element of the wave as the centre of an elementary disturbance, which diverges spherically from that element, and finding by integration the aggregate disturbance at any point in front of the primary wave. With the exception of one case

of diffraction, which will be mentioned further on, the illumination in front of an aperture is insensible except in the immediate neighbourhood of a normal to the primary wave drawn through a point in the aperture. Consequently we are only concerned with the law of disturbance in that part of a secondary wave which lies very near the normal to the primary wave; the nature of the disturbance in other directions does not affect the result, since the secondary waves neutralize each other by interference. Now it has been shown by others, by indirect methods, that if c be the coefficient of vibration in the incident light, dS an element of the area of the aperture, r the radius of a secondary wave diverging from dS, λ the wave length, the coefficient of vibration in the secondary wave will be $\frac{c\,dS}{\lambda r}$, and the phase of vibration must be accelerated by a quarter of an undulation; or in other words, $\frac{\lambda}{4}$ must be subtracted from the retardation due to the radius r. These results, however, according to what has been already remarked, only apply to that portion of a secondary wave which lies immediately about the normal to the primary. The object of the author in this paper was to determine, on purely dynamical principles, the law of disturbance *in any direction* in a secondary wave.

The author has treated the æther as an elastic solid; and as such it must be treated in considering light, if the theory of transverse vibrations be not rejected. The object which he had in view required the solution, in the first instance, of the following problem;—to determine the disturbance at any point of an elastic medium, and at any time, due to a given small arbitrary disturbance confined to a finite portion of the medium. This problem was solved long ago by Poisson; but the author has given a totally different solution of it, which appears to be in some respects simpler than Poisson's. In the course of the solution, the author was led incidentally to the following very general dynamical theorem.

Let any material system whatsoever, in which the forces acting depend only on the positions of the particles, be slightly disturbed from a position of equilibrium, and then left to itself: then the part of the disturbance at any time which depends on the initial displacements will be got from that which depends on the initial velocities by differentiating with respect to the time, and replacing the arbitrary functions, or arbitrary constants, which express the initial velocities by those which express the corresponding initial displacements. Particular cases of this theorem are of frequent occurrence, but the author is not aware of any writing in which the theorem is enunciated in all its generality.

The problem above-mentioned has been applied by the author to the case of diffraction in the following manner. Conceive a series of plane-waves of plane-polarized light propagated in a direction perpendicular to a fixed mathematical plane P. According to the principle of the superposition of small motions, we have a perfect right to consider the disturbance of the æther in front of the plane

P as the resultant of the elementary disturbances corresponding to the several elements of P. Let it be required to determine the disturbance which corresponds to an elementary portion only of the plane P. In this consists the whole of the dynamical part of the theory of diffraction, if we except the case of diffraction at the common surface of two different media; the rest is a mere question of integration. Let the time t be divided into equal intervals, each equal to τ. The disturbance which is propagated across the plane P during the first interval τ occupies a layer of the medium having a thickness $v\tau$, if v be the velocity of propagation, and consists of a certain velocity and a certain displacement. By the problem above mentioned, we can find by itself the effect of the disturbance which occupies so much only of this layer as corresponds to a given element dS of P. By doing the same for the 2nd, 3rd, &c. intervals τ, and then making the number of such intervals increase and their magnitude decrease indefinitely, we shall get the effect of the disturbance which is continually transmitted across dS. The result is a little complicated, but is much simplified when certain terms are neglected which are only sensible when the radius of the secondary wave is comparable with λ, and which are wholly insensible in the physical applications of the problem. The result thus simplified may be enunciated as follows:—In the enunciation, the term *diffracted ray* is used to denote the disturbance in an elementary portion of a secondary wave, diverging in a given direction from the centre; the plane containing the incident and diffracted rays will be called the *plane of diffraction*, the supplement of the angle between these two rays the *angle of diffraction*, and the plane passing through a ray of plane-polarized light and containing the direction of vibration the *plane of vibration.*

The incident ray being plane-polarized, each diffracted ray will be plane-polarized, and the plane of polarization will be determined by the following law:—*The plane of vibration of the diffracted ray is parallel to the direction of vibration of the incident ray.* The direction of vibration being thus determined, it remains only to specify its magnitude. Let

$$\zeta = c \sin \frac{2\pi}{\lambda}(vt - x)$$

be the displacement in the case of the incident light, ζ' the displacement in the case of the diffracted ray, ζ' being reckoned positive in the direction which makes an acute angle with that in which ζ is reckoned positive. Let r be the radius of the secondary wave diverging from dS, and let r make angles θ with the direction of propagation of the incident ray, and ϕ with the direction of vibration; then

$$\zeta' = \frac{cdS}{2\lambda r}(1 + \cos\theta)\sin\phi\cos\frac{2\pi}{\lambda}(vt - r) \quad . \quad . \quad . \quad (a.)$$

When an arbitrary function of $vt-x$, $f(vt-x)$ occurs in ζ, it is not $f(vt-r)$ but $f'(vt-r)$ that appears in ζ', where f' denotes the derivative of f, and accordingly in the particular case in which

$f(u) =$ sin u the line in ζ is replaced in ζ' by a cosine. It may readily be verified, that if the formula (a.) be applied to determine by integration the disturbance which corresponds to the whole of the plane P, the disturbance in front is the same as if the wave had not been supposed broken up, and no disturbance is propagated backwards.

The law obtained for determining the position of the plane of polarization of the diffracted ray seems to lead to a crucial experiment for deciding between the two rival theories between the directions of vibration in plane-polarized light. Suppose the incident light polarized by transmission through a Nicol's prism mounted in a graduated instrument, and let the diffracted light be analysed in a similar manner. By means of the graduation of the polarizer, we can turn the plane of polarization of the incident ray, and consequently the plane of vibration, which is either parallel or perpendicular to the plane of polarization, round through equal angles of say 5° or 10° at a time. According to theory, the *planes of vibration* of the diffracted ray will not be distributed uniformly, but will be crowded towards the perpendicular to the plane of diffraction. But experiment will enable us to decide whether the *planes of polarization* are crowded towards the plane of diffraction or towards the perpendicular to the plane of diffraction, and we shall accordingly be led to conclude, either that the vibrations are perpendicular, or that they are parallel to the plane of polarization.

In ordinary cases of diffraction, the illumination, in consequence of interference, is insensible beyond a small angle of diffraction. It is only by means of a fine grating that we can procure light of considerable intensity that has been diffracted at a large angle. The author has been enabled to perform the experiment, or rather a modification of it, by the kindness of his friends Professors Miller and O'Brien; of whom the former lent him a fine glass-grating, consisting of a glass plate on which parallel and equidistant lines had been ruled with a diamond at the rate of 1300 to the inch, and the latter lent him the graduated instruments required. The theory does not quite meet the case of a glass-grating, in which the diffraction takes place at the common surface of two media, but it leads to a definite result on each of the two extreme suppositions:—1st, that the diffraction takes place before the light reaches the grooves; 2nd, that it takes place after the light has passed them; and the results are very different according as one or other of the two rival theories is adopted. In the principal experiments, the plane of the plate was placed perpendicular to the incident light, and the light observed was that which had been diffracted by transmission through the plate. The angle of diffraction, by which is meant the angle measured in air, ranged in the different experiments from about 20° to 60°. The result obtained was, that when the grooved face was turned towards the eye, there was a very sensible crowding of the planes of polarization of the diffracted light towards the plane of diffraction. When the grooved face was turned towards the incident light, there was a considerable crowding in the same direction, much more than in the other case. Since the effect of refraction, con-

sidered apart from diffraction, would be to crowd the planes in the contrary direction, the result seemed decisive in favour of Fresnel's hypothesis, that the vibrations are perpendicular to the plane of polarization. On the other hypothesis, diffraction would have conspired with refraction to produce a large crowding in a direction contrary to that in which the observed crowding took place. The amount of crowding, in both positions of the plate, was nearly what would be given by theory, on adopting Fresnel's hypothesis, and supposing that the diffraction took place *before* the light reached the grooves, but appeared in both cases little less. The difference, however, was comprised within the limits of uncertainty depending upon the errors of observation and the error in the assumed value of the refractive index of the glass plate.

December 10.—Impact on Elastic Beams. By Homersham Cox, Esq., B.A., Jesus College.

Among the experiments instituted by the Royal Commission appointed to inquire respecting the use of iron in railway structure, was a series relating to impact on beams. These experiments were undertaken by Professor Hodgkinson, and were conducted in the following manner. The two ends of the beam were fixed in a horizontal position, and the blow was given against one of its vertical sides in a horizontal direction. The instrument for giving the blow was a heavy iron ball, hanging down, when at rest, from a point of suspension vertically above the centre of the beam. The ball was raised through different arcs, and after descending by its own gravity, struck the beam. The deflection corresponding to different arcs of descent were carefully noted by a graduated scale.

The object of the present paper is to show that the results might have been predicted by known theoretical principles with considerable precision and confidence. The problem is divided into two parts:—1st, to estimate the amount of velocity lost by the ball at the first instant of collision; 2nd, to ascertain the effect of the elastic forces of the beam in destroying the *vis viva* which the whole system has immediately after collision. In the first part of the investigation, a general formula, derived from the combination of D'Alembert's principle and that of virtual velocities, is given for the motion of any material system subject to impact. The requisite geometrical condition required for the application of this general formula to the present case is obtained by the assumption, that immediately after impact the form of the beam is a gradual and tolerably uniform curve, such as, for example, the elastic curve of equilibrium. In this way it is determined that about one-half the inertia of the beam is effectively applied at the instant of collision to retard the ball.

The *vis viva* of the whole system thus computed is destroyed by the elastic forces of the beam developed by deflection. These, in the second part of the problem, are assumed to vary as the amount of central deflection. By the principle of *vis viva* a formula is easily obtained, connecting the amount of total deflection with the *vis viva* of the system immediately after collision.

Tables are given in which the theoretical and experimental results

are compared. The correspondence is of the closest and most satisfactory nature. Indeed the theoretical result generally differs less from the mean of several experiments than those experiments differ among themselves. Both in the theoretical and experimental inquiries, every possible variation of the elements of the investigation —the relative masses of the beam and ball—the velocity of the latter —the rigidity and dimensions of the former—have been included.

XXXI. *Intelligence and Miscellaneous Articles.*

METEORITE IN NORTH CAROLINA.

On the authority of a communication from J. H. Gibbon, Esq., of the Branch Mint of the United States at Charlotte, North Carolina, we give a condensed view of facts regarding a fall of meteoric masses in that state, not having room for the less important details.

On Wednesday, the 31st of October, 1849, at 3 o'clock P.M., several persons in the town of Charlotte were astonished, and not a few were exceedingly terrified, by a sudden explosion, followed at short intervals by two other reports, and by a rumbling in the air to the east and south.

The sounds were distinct, and continued more than half a minute; they were imputed by some to thunder; but there were no clouds, the evening was calm and mild like the Indian summer, and only a mist was seen in the eastern horizon; nor were the impressions of others better founded, that the explosions were due to the blasting of rocks on a railroad: but sheriff Alexander having once before witnessed the explosion of a meteor, justly traced the detonation to that cause.

The negroes, who are very acute observers of sounds in the open air, denied the thunder, and an old fisherman said that the reports were like those of three pieces of heavy artillery followed by the base-drum. Horses both in harness and under the saddle started with alarm.

Inquiry began to be made for fallen stones, and on Monday a servant of the Mint brought in a report from the county of Cabaras, twenty-five miles distant, that there were notices stuck up on the trees, inviting people to come and see "a wonderful rock that had fallen from the skies on the plantation of Mr. Hiram Post."

Mr. Gibbon of the Mint, with Dr. Andrews, travelled twenty-one miles, and partly at night by torch-light, to see "the large mass of metallic rock." They found placed in a conspicuous position upon a barrel elevated upon a post*, "a bluish gritty rock," of irregular form, eight inches long, six broad and four thick, bearing marks in spots, of recent fracture, but otherwise black as if it had been exposed

* With laudable liberality and caution joined, the worthy proprietor of the boon which had fallen on his land—had annexed a written notice—"Gentlemen, sirs—please not to break this rock, which fell from the skies, and weighs 19½ pounds.—HIRAM POST."

to heat and smoke, the black colour being relieved where the crust had been broken, and a little of the clayey soil in which it was buried in its descent still adhered to it. It had the curved indentations usual in meteorites, as if it had been soft and had yielded to impressions; and lustrous metallic points appeared through the ground colour, which had generally a bluish slaty appearance, but no such rock was known in the neighbourhood. Mr. Post took the travellers by torch-light to see the place where the mass fell. He was at the time in company with a young man on horseback; they heard overhead a whizzing sound; the whole atmosphere appeared to be in commotion; they compared the sound to that of chain shot, or of platoon firing. Nothing was visible; but their attention being directed by the sound towards a large pine-tree east of them, they heard the stone strike "with a dull, heavy jar of the ground," while the dog, in terror, crouched at his master's feet.

Mr. Post (in his peculiar language) had *sighted* the sound, and his negro man ploughing in a field had done the same from a different direction, and by ranging with the aid of these intersecting lines, they the next morning found the stone, which had splintered a pine log lying on the ground; by sounding with a sharp stick in the hole made by the stone in its fall, they soon found it, and extricated it from its hiding-place, which was ten inches below the surface; the dried leaves which had been "driven about by the percussion," aided in discovering the spot, about three hundred yards from the place where Mr. Post had stood at the moment of the fall, which was in the woods, but there were no marks on the trees, although the impression was that numerous small bodies had fallen, "making a noise like hot rocks thrown into the water."

Mr. Gibbon and his companion viewed the place both by torch and daylight, and were convinced of the accuracy of the statement.

The people of the vicinity imagined that a rock had been thrown up from a volcano or from blasting, or had come from the moon, and were not easily persuaded that it could be formed in the atmosphere.

As is usual in cases of extraordinary celestial phænomena, some were terrified by the supposed approach of the day of judgement, or of war, or some other dire calamity; and a militia colonel, in a spirit quite professional, said that "there must be war in heaven, for they were throwing rocks."

At the request of Dr. Andrews, the stone was diverted from another destination, in favour of Prof. Charles U. Shepard, of the Medical College of South Carolina at Charleston, from whom we learn that at a recent date the specimen had not yet reached him.

In due time we shall have the result of his scientific examination; but from the circumstances we have no hesitation in admitting this case as genuine: the facts are perfectly familiar to hundreds on record, and in many particulars are in accordance with the remarkable event of this nature which happened in Weston, Connecticut, in December, 1807, and with which the senior editor of this Journal, with his college colleague, Prof. Kingsley, was at the time familiar. There is no room to discuss theories, but we feel fully assured that

aërolites are not formed in our atmosphere, are not projected from terrestrial or lunar volcanoes, but have a foreign origin, giving us the only reports of the physical constitution of other worlds which have ever reached our earth.

By an additional communication from J. H. Gibbon, Esq., dated November 29, 1849, it is rendered probable that "luminous materials were seen advancing from several points in the atmosphere towards a common centre, where a solid mass of heated metal (materials) exploded and was violently projected in different directions to the earth."

It is stated also that there was a distinct appearance of a single fiery elongated body, like iron raised to a white heat, sparkling in its passage from west to east, rising like a rocket but not vertically, and passing through the air with a long white streak or tail following a denser body in the form of a ball of fire*.

Still it is to be observed, that neither the fire-ball nor any light was seen by many who heard the successive reports and the fall of the stones, and the rumbling "like loaded wagons jolting down a rocky hill†;" but this is no way extraordinary, as it was daytime, with a clear sky, and those only would see the fire-ball who were looking in the proper direction at the time "when it was in its most ardent state." At the explosion, the meteor was about 45° high.

The estimation of time between the disappearance of the light and the arrival of the sound was very different, as made by different persons, at several minutes, even as high as five. The latter supposition would make the meteor almost extra-atmospheric, but doubtless the period of five minutes is much too high, and we infer that the meteor, like that at Weston, was fully within the atmosphere, and probably not over fifteen or twenty miles from the earth when it exploded. It was seen through 250 miles from the line of Virginia, to Sumpter district in South Carolina, and from east to west it was seen through sixty miles.—Silliman's *Journal*, January 1850.

GOLD ON THE FARM OF SAMUEL ELLIOT, MONTGOMERY COUNTY, MD., THIRTY MILES FROM BALTIMORE.

The locality has been known but a few months, and appears to be valuable. Three samples, examined at the mint, yielded as follows:—

No. 1	yielded at the rate of	744	grains per cwt. of ore, or	$610 00 per ton.
No. 2	...	960	...	787 20 ...
No. 3	...	206	...	168 80 ...
Average..............		636		522

The quartz which forms the matrix of the gold crops out amidst a decomposed talcose slate, so that quarrying is very easy. Ores of copper and iron are also present.

* "The true flaming sword of antiquity."

† This was the very comparison used at Weston, in December 1807, by the people there, in describing a portion of the reports heard on that occasion.

Messrs. Bowman and Ebbett of New York state that much gold appears to be disseminated throughout the gangue, in so minute a state of division as to be invisible to the naked eye.—*Proc. Amer. Phil. Soc.* 1849, p. 85.

GOLD OF CALIFORNIA. BY C. S. LYMAN.

The gold the past season has turned out much better than was expected. Many rich deposits in all parts of the mines have been opened. On the middle fork of the Rio de los Americanos, two men recently dug $28,000 in two months. I saw a portion of it in lumps of the size of hen's eggs, and larger. The Mariposa has yielded several similar prizes, and so has the Mokelemnes. But for these few fortunate diggers, there are thousands who scarce earn a dollar a day. From the best information I can get, industrious workers have not averaged more than eight or ten dollars a day—some estimate it much lower; multitudes do not pay expenses, particularly clerks, professional men, and others unaccustomed to hard work.

The gold has at last been discovered in place—in veins penetrating quartz beds—on the Mokelemnes, and in the vicinity of the Mariposa and one or two other places. I have this from gentlemen who have seen the veins, and who are reliable witnesses. These veins are of course not worked yet, as it is more profitable to dig the wash gold. One of these veins has been "denounced" (as it is termed) under the Mexican laws, by Mr. Fremont. The working of the innumerable rich veins, which undoubtedly will be opened in the mountains, will constitute an immense and profitable mining business for centuries. I have no fear that the gold, as many imagine, will all be dug out in a year or two.—Silliman's *Journal*, January 1850.

ON THE OIL OF ELEMI. BY M. H. DEVILLE.

The author states that M. de Bonastre subjected the resin of elemi to distillation with water and obtained an oily principle, respecting which he merely stated that it had been produced.

Some time since Mr. Stenhouse published a series of experiments on the oils of olibanum and elemi.

There are several varieties of elemi which differ from each other in consistence and the quantity of ligneous matter accidentally and intimately intermixed. This resin is sometimes as soft as thick honey, and at other times solid and hard, according to the degree of alteration which it has undergone by exposure to the air. It will therefore be readily conceived that the quantities of oil of elemi produced by distilling the resin may vary, as proved by the numbers of Mr. Stenhouse, M. De Bonastre and the author's, very considerably. Some specimens of elemi of good quality yielded M. Deville more than 13 per cent. of oil.

After the usual purifications, which are very readily effected, the

oil is a colourless liquid of great limpidity and fluidity, and a pleasant odour; its density at about 52° F. is 0·849; its mean index of refraction is 1·4719 at 56°; that is to say, it is the same as that of oil of turpentine and the greater number of its isomeric compounds. Its rotary power is 90°·30, it is consequently one of the substances which turns most strongly to the left.

Its boiling-point is remarkably fixed (which, the author remarks, all essential oils do not possess), and is about 345° F. under a pressure of 755 millimetres. M. Deville found the oil of elemi, like Mr. Stenhouse, to be similarly composed with the oil of turpentine, lemons, &c.:

	I.	II.	
Hydrogen	11·9	11·9	11·76 } $C^{20}H^{16}$
Carbon	88·0	88·1	11·76 }
Loss	·1		
	100·0	100·0	100·00

The density of the vapour was found to be the same as that of oil of turpentine; by experiment it was 4·84; calculation gives 4·76. Contrary to the observation of Mr. Stenhouse, the author was able to obtain two camphors of elemi, one of which is solid and crystalline and the other liquid. Both have the same composition, and are isomeric with the camphors of lemon. The following are the author's analyses:—

	Experiment.			Calculation. ($C^{16}H^{8}$, HCl)
Carbon	57·3	57·3	57·5	57·4
Hydrogen	8·7	8·7	8·7	8·6
Chlorine	34·0	34·0	33·8	34·0
	100·0	100·0	100·0	100·0

The quantity of hydrochloric acid absorbed by oil of elemi is considerable, amounting to 47·7 per cent. of the weight of the oil; and in order to obtain the crystallized camphor, the current of gas must be continued long after the period at which the saturation appears to be complete. The matter is then liquid, but the solid camphor is readily deposited after the excess of acid has escaped by exposure to the air. The rotation of this camphor is nil, like that of its isomer, camphor of lemons.—*Ann. de Ch. et de Phys.*, Septembre 1849.

ACTION OF NITRIC ACID ON RHUBARB. BY M. GAROT.

It results from the author's experiments, that when one part of rhubarb is treated with four parts of nitric acid, there remains a peculiar substance unacted upon by the acid, amounting to 8 or 10 per cent. in indigenous rhubarb, and 15 to 20 per cent. in foreign.

This matter, to which it is proposed to give the name of *erythrosin*, is yellow when obtained from indigenous rhubarb, and orange-

coloured from foreign rhubarb. It is totally soluble in alcohol and æther, which by evaporation leave rhabarbaric or erythrosic acid, forming with the alkalies red or purple compounds applicable to the arts and to pharmacy. After drying, it has the appearance of agglomerated powder of rhubarb; when beaten in a mortar, the particles flatten and acquire a strong shining appearance. This substance is perfectly tasteless, the odour slightly aromatic and nitrous, probably owing to the presence of a little nitric acid, which repeated washings are incapable of separating.

When heated in a glass tube, abundant yellow vapours of rhabarbaric acid are produced, which soon condense into a yellowish film in the tube, leaving a white residue of lime, which is stronger in foreign erythrosin than in indigenous.

Water has but little action on erythrosin; it becomes merely slightly yellow; but if it be heated to ebullition, it acquires a reddish amber tint, and becomes slightly acid. This acidity appears to be owing to a small quantity of nitric acid, unremoved by repeated washings; for it cannot be attributed to rhabarbaric acid, the quantity of which is very small. When evaporated to a certain extent, the liquor deposits gelatinous matter, which has all the characters of pectin.

When cold, alcohol acts but slightly on erythrosin, acquiring merely an amber tint; but if it be boiled, the colour deepens and acquires the reddish tint of Malaga wine. It requires seven or eight times boiling to deprive erythrosin of all soluble matter. The first and last washings redden litmus paper.

As evaporation proceeds, alcohol deposits yellowish flocculi, pellicles of a crystalline appearance forming on the surface: but crystals do not form on cooling; a dry product can be obtained only by evaporation to dryness. This so obtained has the form of granular pulverulent matter, of a brownish yellow colour, and somewhat resembling Spanish snuff in appearance: its smell is slightly aromatic; its taste rather acid and mucilaginous, but not at all like that of rhubarb.

This matter, when repeatedly treated with æther and heated, totally dissolves; by evaporation a sulphur-coloured granular substance is obtained, which has a slightly aromatic odour, and the surface of which, by exposure to the air, acquires an orange tint: it possesses all the physical and the principal chemical characters attributed to rhabarbaric acid by Brandes, and to rhabarbarin by Geiger.

Indigenous and foreign rhubarb yielded perfectly similar products, excepting that indigenous rhubarb gave most. Of two samples tried, the foreign gave 60 per cent. and the indigenous 85 per cent. of erythrosin; but this did not occur in all cases.

When a gramme of erythrosin is added to half a gramme of potash dissolved in fifteen grammes of water, the liquor soon becomes of a reddish purple colour, which is more or less intense according to the kind of rhubarb employed; some days are required to complete the operation. Its colouring power is so great, that one gramme of erythrosin is sufficient to impart a more brilliant colour

to one litre and a half of alcohol, than six times the quantity of cochineal.

Erythrosate of ammonia, after evaporating the excess of ammonia, possesses the same properties as the potash salt; but its colouring power is four times greater, and may be advantageously employed as red ink and for colouring soaps.

It appears that Russian rhubarb possesses the greatest colouring power, then Chinese, and lastly indigenous rhubarb; but the last-mentioned affords the most brilliant colour as a dye.—*Journ. de Pharm. et de Chém.*, Janvier 1850.

ACTION OF PHOSPHORIC ACID ON CHOLESTRINE. BY M. C. ZWENGER.

Cholestrine is readily decomposed by concentrated phosphoric acid. The products of this decomposition, like those obtained by the action of sulphuric acid, are solid and well-defined carburetted hydrogens; but by their physical properties they are clearly distinguished from those obtained with sulphuric acid.

When one part of cholestrine is treated with six or eight parts of concentrated phosphoric acid, and the mixture is evaporated till its temperature reaches 278° F., the fusing point of cholestrine, this substance is completely decomposed. On the cooling of the fused mass, which must not have its temperature increased, a matter is obtained which has lost all crystalline appearance, and contains two carburetted hydrogens, *cholesteron* and *cholestearon*; these two carburets are separated by boiling alcohol, which dissolves the cholesteron. After purification by repeated crystallizations from absolute alcohol, this substance is obtained in brilliant rhombic prisms which fuse at 154° F. At a higher temperature the cholesteron boils, and may be distilled almost unaltered. It burns with a sooty flame; nitric acid oxidizes it, and sulphuric acid renders it of a red colour. It is insoluble in water, slightly soluble in cold alcohol, more soluble in it when concentrated and boiling, and very soluble in æther. Its mean composition is—

Carbon	87·78
Hydrogen	12·22
	100·00

Cholestearon is obtained in boiling the residue which has been exhausted by alcohol in æther. By cooling and evaporation of the æthereal solution, the new carburetted hydrogen is deposited in the form of a crystalline mass, which is readily purified by repeated solution in æther.

Cholestearon crystallizes from its æthereal solution in white silky needles, forming an interwoven mass. It dissolves with difficulty in æther, very slightly in alcohol, and not at all in water. Its proper solvents are the fixed oils. It fuses at about 347° F. At a higher temperature it distils, undergoing alteration. It burns with a sooty

flame. Its composition is expressed by the following numbers:—

Carbon	87·76
Hydrogen	12·24
	100·00

It is consequently isomeric with cholesteron.—*Journ. de Pharm. et de Chém.*, Novembre 1849.

METEOROLOGICAL OBSERVATIONS FOR JAN. 1850.

Chiswick.—January 1. Sharp frost: fine: cloudy. 2. Hazy: fine. 3. Foggy: hazy. 4. Foggy: overcast: clear. 5, 6. Frosty: very fine: clear and frosty. 7. Sharp frost: clear: severe frost at night. 8. Frosty: overcast. 9. Slight fall of granular snow: overcast. 10. Snowing slightly: overcast. 11. Hazy throughout. 12. Slight snow: dusky: hazy. 13. Hazy: clear and frosty. 14, 15. Cloudy and cold. 16, 17. Densely overcast. 18. Foggy: snow at night, with heavy rain. 19. Cloudy: drizzly. 20. Frosty. 21. Cloudy. 22. Hazy. 23. Hazy: clear at night. 24. Foggy and drizzly. 25. Foggy: densely overcast: rain at night. 26. Densely clouded: showery. 27. Sudden rise of barometer: frosty: very fine. 28. Overcast. 29. Very fine: rain. 30. Foggy: very fine. 31. Hazy and cold: heavy rain at night.

Mean temperature of the month	33°·11
Mean temperature of Jan. 1849	39 ·56
Mean temperature of Jan. for the last twenty-four years .	36 ·60
Average amount of rain in Jan.	1·60 inch.

Boston.—Jan. 1. Fine. 2. Cloudy. 3, 4 Cloudy: rain P.M. 5—8. Fine. 9—11. Cloudy. 12. Cloudy: snow A.M. 13. Fine. 14—17. Cloudy. 18. Cloudy: snow and rain P.M. 19. Cloudy: rain early A.M. 20. Cloudy: snow A.M. 21, 22. Cloudy. 23. Fine. 24. Cloudy. 25. Fine. 26. Cloudy: rain and snow P.M. 27. Fine. 28. Cloudy. 29, 30. Fine. 31. Fine: rain P.M.—N.B. This has been the coldest January since the year 1838.

Applegarth Manse, Dumfries-shire.—Jan. 1. Frost: dull and threatening change. 2. Thaw: small rain. 3. Thaw: drizzle. 4. Slight frost early A.M.: rain and wind P.M. 5. Snow half an inch deep: frost. 6. Frost very hard: snow lying. 7, 8. Frost very hard. 9. Frost very hard: thermometer 11°. 10. Cloudy, looking like change. 11. Cloudy, but still freezing. 12. Still slight frost, but unsettled. 13. Frost still slight: cloudy. 14. Frost, slight A.M.: harder P.M. 15. Bright and clear: hard frost: a little snow. 16. Frost: slight shower of snow: looking dull. 17. Hard frost: clear: snow lying. 18. Hard frost, and heavy snow 4 inches deep. 19. Fro t not so hard: thaw P.M.: frost again. 20. Frost: additional sprinkling of snow. 21. Frost hard again. 22. Frost moderate. 23. Thaw: mild: cloudy. 24. Thaw: snow melting fast. 25. Rain in the night: thaw continuing. 26. Thaw A.M.: came on to freeze at 6 P.M. 27. Hard frost A.M.: thaw and rain P.M. 28. Heavy rain: snow nearly gone. 29. Slight frost A.M.: keener P.M. 30. Frost moderate: snow all gone. 31. Slight shower of snow A.M.: frost: rain P.M.

Mean temperature of the month	30°·8
Mean temperature of Jan. 1849	36 ·3
Mean temperature of Jan. for the last twenty-eight years.	34 ·9
Average amount of rain in Jan. for the last twenty years.	2·60 inches.

Sandwick Manse, Orkney.—Jan. 1. Cloudy. 2. Bright: fine: rain. 3. Showers: sleet-showers. 4. Thunder: sleet-showers. 5. Thunder: showers. 6. Sleet-showers. 7. Sleet-showers: clear. 8. Cloudy. 9. Rain: cloudy. 10. Cloudy: sleet-showers. 11. Cloudy: clear. 12. Snow-showers: cloudy. 13. Cloudy: clear: frost: aurora. 14. Snow-showers: snow-drift. 15. Snow-drift. 16. Snow-drift: snow: thaw. 17. Rain: clear: frost: aurora. 18. Clear: frost: cloudy. 19. Snow-showers: cloudy: aurora. 20. Cloudy. 21. Cloudy: thaw. 22. Showers: drizzle. 23, 24. Drizzle. 25. Cloudy: snow-showers. 26. Snow-showers: snow: clear. 27. Snow: frost: snow-showers. 28. Showers: drizzle. 29. Bright: large halo. 30. Fine: frost: clear: aurora. 31. Bright: rain.

Meteorological Observations made by Mr. Thompson *at the Garden of the Horticultural Society at* CHISWICK, *near London; by* Mr. Veall, *at* BOSTON; *by the* Rev. W. Dunbar, *at Applegarth Manse,* DUMFRIES-SHIRE; *and by the* Rev. C. Clouston, *at Sandwick Manse,* ORKNEY.

Days of Month.	Barometer.							Thermometer.								Wind.				Rain.			
	Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.
1850. Jan.	Max.	Min.		9 a.m.	9 p.m.	9¼ a.m.	8½ p.m.	Max.	Min.		Max.	Min.	9¼ a.m.	8½ p.m.								
1.	30·271	30·213	29·97	30·13	30·12	30·14	30·09	35	19	27·5	34	26	45	41	w.	nw.	nw.	s.	......	......	......	......
2.	30·223	30·221	29·96	30·08	30·06	29·98	29·87	39	27	31	40	30	42	43	nw.	nw.	sw.	ssw.	......	......	......	......
3.	30·201	29·996	29·84	29·89	29·70	29·63	29·48	44	38	33	42	40	45	42	nw.	ssw.	wsw.	w.	·02	......	......	·22
4.	29·737	29·445	29·39	29·33	29·15	29·32	29·07	48	26	36	43	34½	40½	38	sw.	w.	se.	w.	......	·07	......	·40
☾ 5.	29·437	29·127	29·13	29·18	29·26	29·07	29·27	42	21	32	40	31	37½	37½	w.	w.	e.	n.	......	·03	......	·25
6.	29·599	29·424	29·22	29·36	29·55	29·40	29·60	35	18	27·5	29	13½	35	37½	e.	nw.	ne.	n.	......	......	......	·19
7.	30·154	29·867	29·62	29·80	30·07	29·94	30·09	36	14	23	27	13	37½	33	n.	nnw.	se.	s.	......	......	......	·15
8.	30·330	30·332	29·03	30·20	30·20	30·08	30·09	36	31	31	25	13	39	41	ne.	n.	e.	sse.	......	......	0·05	·03
9.	30·211	30·065	29·87	30·08	30·05	30·03	30·12	33	29	33	23½	11	37	38	ne.	n.	e.	sse.	......	......	......	·04
10.	29·908	29·824	29·64	29·87	29·65	29·95	29·75	34	28	33	31	20½	37	36½	ne.	n.	ne.	se.	......	......	......	·04
11.	29·791	29·760	29·54	29·63	29·78	29·75	30·01	34	27	31·5	31½	29	36½	34½	se.	calm	e.	sse.	......	......	......	·16
12.	29·922	29·883	29·65	29·90	29·93	30·17	30·27	32	27	30	34½	30	37	37	ne.	e.	e.	se.	......	......	......	·11
● 13.	29·912	29·882	29·77	30·00	29·97	30·29	30·27	31	22	27	33½	30½	35½	30	ne.	e.	e.	ene.	......	·30	......	......
14.	29·797	29·544	29·65	29·83	29·77	30·08	30·10	31	26	30	33½	30½	34	28½	e.	e.	e.	e.	......	......	......	·04
15.	29·433	29·390	29·30	29·63	29·51	29·98	29·92	31	25	27·5	28½	19	27½	31	e.	e.	e.	e.	......	......	......	......
16.	29·643	29·143	29·24	29·41	29·55	29·78	29·81	33	29	30	33½	26½	32	36	ne.	calm	e.	e.	......	......	0·05	......
17.	29·944	29·793	29·57	29·73	29·80	29·82	29·86	36	28	33	32	14	35	34	n.	nw.	ne.	se.	......	......	......	·57
18.	29·984	29·615	29·68	29·73	29·48	29·83	29·76	35	31	30	32	13½	32	35	se.	calm	e.	e.	·55	......	......	......
19.	29·724	29·437	29·16	29·29	29·70	29·76	30·00	45	27	33	35½	30½	35	33	w.	e.	e.	ese.	·02	·70	......	·09
20.	30·199	30·047	29·84	29·99	30·05	30·16	30·15	30	27	30	32	29	33½	34½	ne.	e.	ese.	sse.	......	......	......	·02
☽ 21.	30·380	30·227	30·05	30·09	30·19	30·08	30·10	34	26	27	32	25½	35	38	ne.	e.	ese.	sw.	......	·10	......	·02
22.	30·485	30·481	30·20	30·23	30·24	29·97	30·03	36	31	29	36½	29	44	47	se.	e.	ese.	wsw.	......	......	......	·27
23.	30·449	30·397	30·10	30·21	30·22	30·05	30·13	40	25	30	41	35	47	46	sw.	w.	sw.	w.	......	......	0·30	·05
24.	30·370	30·168	30·00	30·15	29·96	29·93	29·81	36	31	30	40	35½	46	47	w.	w.	sw.	w.	·01	......	......	·08
25.	29·993	29·724	29·60	29·60	29·43	29·60	29·49	51	45	33	41	36	43	34	sw.	wsw.	n.–w.	e.	·14	......	......	·06
26.	29·874	29·384	28·96	29·21	29·92	29·71	30·11	51	21	44	41	36	28½	31	w.	w.	ene.	n.	·12	......	......	·43
27.	30·452	30·316	30·16	30·27	29·94	30·22	29·77	37	29	23·5	34½	22½	34½	32	sw.	nnw.	se.	s.	......	·19	......	......
○ 28.	30·089	29·803	29·60	29·55	29·47	29·08	29·24	49	33	36	46	32½	47	41	sw.	sse.	sw.	n.	·11	......	......	·55
29.	30·003	29·879	29·56	29·82	29·96	29·93	30·10	50	32	43	45	34	37½	32	ne.	wnw.	e.	se.	·08	......	......	·04
30.	30·302	30·173	29·91	30·05	30·07	30·15	30·18	42	24	30	37	29	32	30	e.	e.	w.	se.	......	......	1·50	......
31.	30·190	29·900	29·90	29·90	29·50	29·99	29·34	50	40	30	43½	31½	37	39	se.	calm	se.	sse.	·38	......	......	......
Mean.	30·032	29·831	29·65	29·811	29·814	29·866	29·867	38·58	27·64	31·1	35·4	26·3	37·58	36·70					1·43	1·39	1·90	3·81

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XXXII. *Additional Observations on the Meteor of February* 11, 1850, *and Deduction of the Results from all the Observations.* *By* James Glaisher, *Esq., F.R.S., F.R.A.S., and of the Royal Observatory, Greenwich.**

IN my previous paper upon the meteor of February 11, 1850, I gave all the accounts I had then received upon it. Since that time I have been favoured with many more on the same subject; and several gentlemen have been kind enough to revisit their places of observation, and to take such measures as to confirm the accuracy of their first accounts or to correct them. I have thus been enabled to form a better judgement of the path, &c. of the meteor. I shall first give the results of my inquiries: it will be found that some are contradictory, but I have been fearful to omit them, as by so doing I might pass over facts which may become valuable.

The numbering of the accounts are in continuation of those in the preceding paper.

XXIV. Penzance, Cornwall. George Davies, Esq., Commander R.N., Coast Guard Service, favoured me with the following:—

"Being out on duty, and about half-way between this place and St. Ives, my attention was arrested by the meteor, and the exclamation of the mounted guard who accompanied me of 'Good God! what's that?' The night being here somewhat misty, the meteor did not present that definite rocket-like appearance described by others, but of an immense globular expansion of flame seen at a great altitude in the north, and taking its course from N.W. to S.W., dissolving with a momentary increased brilliancy. No report was heard. It was visible from 15^s to 20^s, and the time was about $11^h\ 45^m$ P.M."

* Communicated by the Author.

I requested Commander Davies to measure the altitude, and he sent me the following letter :—

XXV. "I think if I were to say anything as to the altitude of the meteor in degrees from the horizon, I might only incorrectly disturb your calculations, as I was in fact at its appearance in a sort of brown study, and aroused from it by the ejaculation of my mounted guard, more alive to the brilliant effect than observant of the object itself: what I said as to its great altitude would have been more correctly rendered by 'great distance,' inferred mostly from the absence of any heard report here. I wrote in haste (as I was so far behind hand), merely to let you know that it was at all seen so far west, and think that mere fact will lead you to deductions more correct than any real data I can give. I have no copy of what I wrote, but think you will find that I said from S.E. to N.W. as its direction; and that was my impression, for we were facing very nearly north, and the mounted guard says that it was from 'rather the back of his right shoulder to the front of his left;' though, as I said before, you will do better to consider our observations as simply confined to its intense illuminative effect at this extreme west point."

XXVI. From a Lady at East Budleigh, a private communication to myself.

"I reside within two or three miles of Budleigh Salterton, a small watering-place close to the sea, between Sidmouth and Exmouth. Having heard that the meteor had been seen at Budleigh Salterton, I have been anxious to obtain information about it; and yesterday I saw a sailor belonging to the Coast Guard who, I heard, had seen it. I found out this man, and requested him to describe the appearance of the phænomenon. He said he was on duty on the evening of the 11th of February, and about half-past ten o'clock he suddenly observed a brilliant light, which made every object as clear as the daylight; he turned round to see from whence this proceeded, and saw passing on through the sky a *bar* of light, bright and red, from whence quantities of sparks issued from each side and behind: it appeared about a yard in length, and narrow. There was some cloud between him and the meteor, yet he saw it most distinctly for a second: he believes it passed not smoothly, but as if impelled with a sort of jerking movement, he said such as a steam-carriage has; its progress was towards the east in a downward slanting direction; a hill concealed its further progress. He soon met another sailor of the Coast Guard from Exmouth. They immediately talked of what they had just seen, agreeing, that although having been very many years at sea, they had never seen anything like it. The Ex-

mouth man said he saw it when it burst from the sky first. I saw also the father of a young man in Budleigh who saw the meteor, and described it as a bar of light, emitting sparks about a yard long. This young man was in bed, and seeing suddenly a brilliant light, looked out of window and saw what occasioned it."

XXVII. Yeovil, Somersetshire. Second communication from J. Hannane, Esq.

"I find, on further examination, that I have made a great error in representing the altitude of the meteor as 60 degrees, for I find, by testing the point by a theodolite, that the altitude of its first appearance was 36 degrees only, and it was 8 or 9 degrees west of north. I was looking towards the north at the time, being I believe endeavouring to find the polar star. How I came to reckon the angle at 60 degrees, was from having always assumed the ridge of the house over which it appeared, which is 41 feet high, as presenting that angle where I was standing, being a distance of 76 feet, which of course was an error. The course of the meteor was left to right. I lost sight of it finally behind a chimney, and then it was at the altitude of 22 degrees, and east of north 35 degrees*. These have been taken by the instrument.

"Since writing the above, I have taken the bearings by compass only with a neighbour, who also saw it in another part of the town; the elevation is much the same as I noted, but its point of disappearance also behind houses gives 20 degrees east of north. The meteor must have travelled considerably east of north, or I could not have seen its reflected light from the roof of a house near, unless it was from the atmosphere itself reflecting it.

"The great distance it travelled will perhaps account for its gradually increasing in size, as, if it originated over the Bristol Channel, it would have come nearer to this part of the kingdom. No explosion was heard in the neighbourhood."

XXVIII. Langport, Somersetshire. Second communication from William Bond Paul, Esq.

"In accordance with the desire you expressed in your letter of 26th ult., I have visited the spot from which I saw the meteor of the night of February 11, and have taken the altitude and azimuth of the supposed place at which I first observed it, and that at which it disappeared.

"The observations are as follows, viz.—

* The variation of the compass at Yeovil is 23° 20′ nearly; therefore the azimuth was 11° 40′ E. of N.

"Altitude of point where first seen was 56 degrees*, and its azimuth was N.N.E.

"Altitude of point where last seen was 10 degrees, and its azimuth was N.E.

"I should state that I think the meteor must have proceeded some way on its course before I noticed it; for, if I recollect rightly, it was issuing from behind a dark cloud when I first saw it.

"The direction in which I was travelling was N.W.

"I much regret that I cannot vouch for the correctness of my observations, as they have been made entirely from memory, for unfortunately I did not note at the time any fixed object. I trust however that they may be of some little service to you.

"I did not hear the report occasioned by its explosion.

"A lady, a relation of mine, informs me that she was going from Cardiff towards St. Nicholas in Glamorganshire, when her attention was attracted by the meteor; but to be enabled to see it, she was obliged to turn herself half round, as it was behind her, and apparently in the direction of Llandaff. I therefore conclude it must have originated north of Cardiff at least. She states that it appeared to her to exhibit a variety of beautiful and brilliant colours."

XXIX. Bristol. Favoured by Robert Boyd, Esq., M.D.

"Happening to be walking home on Monday last about half-past ten o'clock P.M., I suddenly observed a bright blaze of light, which illuminated the whole atmosphere. On looking instantly round to ascertain its cause, I saw falling in a north-easterly direction, at an angle of about 10 degrees from the perpendicular, what was apparently a ball of fire having a bright luminous tail covering a space of about 15 degrees. It disappeared behind the New Villas opposite Vyvian Terrace. Though I heard no explosion or passing sound, I am much inclined to think it was an aërolite, from its apparent proximity and its great illuminating power, and *not* one of those luminous meteors termed falling stars, which are so often seen in the month of November."

XXX. Bristol. Second communication from Dr. Boyd.

"I went to the spot where I observed the meteor with a mariner's compass, and found that the part where I first observed the meteor bore N.E. by N.†, and the point where it disappeared behind the houses opposite Vyvian Terrace bore N.E. by E†., thus making two points of the compass through

* This altitude is much too great.

† The variation of the compass at Bristol is about 23° 25′; hence the

which the meteor passed. As regards the altitude, I took a carpenter's square rule, to which I attached a plumb-line and a pair of compasses, the leg of which was fixed along the level of the upper horizontal line of the square, and found that the point where the meteor first appeared, according to my recollection, was at an angle from the horizontal line about 40 degrees; the point where it disappeared behind the houses bore an angle of 5 degrees* above the horizontal line; thus the space through which the meteor passed my eye was 35 degrees. From where I stood to the back of the houses where I lost sight of it was about 250 yards, and the height of the top of the houses above where I stood was about 35 feet.

"I have inquired of different parties, but have learnt nothing except from Mr. Leach, proprietor of the Bristol Times newspaper, whom I accidentally saw in his office when calling there. He told me that when going home along St James's Place, Kingsdown, Bristol, a retired quiet street running by the map due north-east, he saw the meteor suddenly appear over the houses on the east side, as if rising, and then disappear behind the houses on the other side of the street.

"The meteor seemed to cross the street obliquely, and just as it was disappearing it burst into many small luminous bodies, then into again smaller, which again burst into others. He (though the spot was perfectly quiet and retired) did not hear any noise of explosion. He told me, however, that a person residing at Brislington, three miles east of Bristol on the Bath road, told him he heard a slight sound. I was anxious to get from Mr. Leach further particulars, as they seemed to me most important; but the above were all I could collect. The meteor certainly passed to the north of Bristol."

XXXI. Reading, favoured by the Rev. Charles Joseph Goodhart.

"I was coming down Castle Hill in this town, and the meteor burst upon me all at once on the opposite side of the way, so that I was able to measure its path by the houses opposite. This, as nearly as I can tell, subtended an angle of about 35 degrees, and I should think at an altitude from 25° to 30°. Its length I should have guessed at about 1° 30′, but

azimuth of the meteor at the time of its first appearance was about 10° E. of N., and at its last appearance about 33° E. of N. The difference between these values is one point, and not two, as mentioned above. I fear the values are not correct.

* This altitude is certainly incorrect. With the azimuth of 10° E. of N., the line of direction of the meteor is cut nearly over Kidderminster. From the given altitude of 40°, its distance from the earth at this time would be fifty-eight miles, which from other observations is too great.

in this I may not be accurate. Its breadth struck me as about that of Venus at her greatest brilliancy, so that it appeared as a line of light such as would arise from the lengthening of that planet to the same extent. Its head might be a little larger, but not much; its colour was a dull red. When it burst I felt sure that the fragments described *parabolic curves*, and I did not notice that any one of these returned upon its path. Its path was *perfectly straight*, and slightly inclined to the horizon.

"It appeared as I stood as nearly as possible to the N.W., and its apparent motion was from S.W to S.E. I did not test its light by anything, but it quite startled me. I should not have said its continuance exceeded 4 seconds. I think it was hardly as much."

At my request Mr. Goodhart again went to the spot, and took the altitudes and azimuths of the points to the best of his recollection. The following is the additional information:—

XXXII. Second communication from the Rev. C. J. Goodhart.

"I am sorry I did not take more pains to certify my statements before I sent them to you. I have now done so; but the distance of time renders me unable to give you the certain information I could wish, though the observations I have made today are, I believe, approximate to the truth. The measurements I have made may be thoroughly depended upon, but the precise spots I can only state within limits. I am not sure to within two or three paces of my own position on the hill, and the limits therefore at the first point of sight, and at the point of explosion, are for the former 28° to 35°* west of the north point, and for the latter 2° to 10°* east of it.

"These measurements were obtained by the comparison of two compasses with the meridian line. The altitude of the former point (where first seen) was *not greater* than 26°; this I think I am sure of; it might not be more than 23° or 24°; and the altitude of the point of explosion I place at 16°, or perhaps a little higher, but not I believe lower.

"These I have obtained by measuring the angles between the lines of sight and a plumb-line."

XXXIII. Euston Square, London. Third communication from G. F. Burder, Esq.

"There is one point, however, in which I think it worth

* The variation of the compass at Reading is about 22° 50′. I do not know the value used in deducing the above values, but they do not seem to be right; we may conclude, however, that at its first appearance it was situated at an angle of about 39° west of the place of explosion.

while to say another word, inasmuch as the accidental circumstances of my position rendered this particular point a matter of absolute certainty. At the time of the occurrence of the meteor the pole-star formed so ready a standard of comparison, that any great error in estimating its position in azimuth would have been impossible; indeed I may say that I am confident it passed the north line*."

XXXIV. Enstone, Oxon. Second communication, in a letter to the Astronomer Royal, from the Rev. J. Jordan.

"In the case of the young man referred to in my former letter, I have obtained, and subjoin for you, the measures sent with this, and which I took by first placing a long pole to the point of the heavens you indicated, fixing that, and then by means of a carpenter's level with plumb-line attached to it, taking the measure of the lines forming the right angle†. This observation was made at Church Enstone, in the county of Oxford. The following observations I have made today at Little Tew, a village just two miles north of this place—Enstone. My informant is a very intelligent young farmer, named Mr. J. Kimber; and his account is exceedingly good and accurate. He relates that on the night in question he was riding slowly home, and having lately attended some lectures on astronomy at Chipping Norton, the sky being at the time brilliant with stars, he was looking up and admiring them, and reflecting on parts of the lecture, when suddenly he saw what seemed to him at first a star of the smallest magnitude fall from the sky and descend rapidly, as if it were about to come down upon himself, when suddenly it turned off at an angle, took a horizontal course, exploded into about eight or ten bright globules, and disappeared. Its appearance was grand and brilliant in the extreme, delineating its path in the heavens distinctly by a long trail of light, and illuminating the whole scene so as to enable him distinctly to observe and note in his mind its whole course. I accompanied him to the spot where he witnessed all this, and in a manner precisely similar to that described above, I took measures of the altitudes of three points; that when it was first seen, that when its path seemed to become horizontal, and lastly at its explosion‡."

* The meteor does not seem to have reached the meridian of London from all the other accounts.

† From these measures the altitude at explosion was 45°.

‡ From these measures the altitude on its first appearance was 62° 17′, and both the other altitudes were 45°.

These altitudes are all too great; they make the meteor when first seen at a distance from the earth exceeding 100 miles, and at the point of explosion at about 60 miles.

XXXV. Padbury, Buckinghamshire, as copied from the *Times* of February 14.

"At a quarter before 11 o'clock last night I first observed the meteor coming from the north-west; it did not appear at this time much larger than falling stars usually are; but it continued its course, every moment increasing in brightness, exhibiting a tail of a blue colour and which was remarkably vivid, until it had reached the east, when it exploded into thousands of brilliant coruscations and disappeared. After an interval of about 90 seconds it was followed by a loud clap of thunder, of a very peculiar crackling nature, of long duration, which passed off into the north-east."

XXXVI. Deddington. Miss Ellen B. Faulkner favoured me with the following communication:—

"As I was fortunate enough to be an observer of the splendid meteor seen on Monday night, Feb. 11th, at a quarter before 11 o'clock, and believing that some account of it would prove interesting to those who did not witness the phænomenon at this place, I send you the following account; but as the garden in which I was standing at the time I saw it was a small one, and surrounded by high walls, I was unable to see its close, although I distinctly saw the commencement of it, as my face was directed to that part of the heavens in which it first appeared, which was W.S.W., and it fell E.N.E. The appearance it first assumed was that of a luminous body much larger than the planet Jupiter, and it rapidly increased in size and brightness as it approached nearer to the earth, leaving behind it as it passed through the heavens a brilliant trail of light. The form of it when descending was conical, the narrow end throwing out several streaks of extreme brilliancy; as it approached the north it made a wavy motion similar to that of a snake.

"When I first observed it the colour was yellow, but just before it began to descend it became blue, and emitted a light so great that the smallest object was discernible. As it disappeared from my sight, the broader end was I believe larger than the moon's disc. The whole time the meteor lasted was about 40 seconds, and four or five minutes after, while standing in the same position, I heard a report something like that of a cannon when at a great distance. I believe the sound came from the northern part of the heavens. Persons who heard the report at Banbury, a town six miles from Deddington, describe it to have been as loud as thunder when at no great distance. At the time the meteor appeared the stars shone very brilliantly, and the evening was particularly clear, but the whole of the day had been wet and stormy."

XXXVII. Deddington. Miss Faulkner's second communication.

"It is with mixed pleasure and hesitation I comply with your request concerning the altitude and azimuth of the meteor. My surprise was so great at its first appearance, that I am unable to give so correct an account as I could wish, as I did not mark its path till now, and have therefore done it from recollection; had I noticed it directly after I saw it, I should feel more sure that it would be correct, but I think from the position of the buildings it may be depended on.

"The place on which I stood was unfavourable for seeing its close, a high wall being on the eastern side of me; but I think, from the time it kept in my sight, that the explosion of the meteor must have taken place immediately after it disappeared from view.

"The noise of the report was very like that produced by the rolling of barrels along the pavement; this idea occurred to several others, who described it to me in a similar manner, particularly at Worton, three miles west of Deddington, where the clergyman of that place informed me it shook his house.

"A person who happened to be on Edge Hill, said that the light emitted was so strong, that it enabled him to see the sheep in the fields most distinctly for a great distance round.

"The azimuth of the meteor on its first appearance was 68° W. of N., and when I lost sight of it was 82° E. of N.*"

XXXVIII. Bedford. Through the kindness of S. C. Whitbread, Esq., Henry John Dodwell, Esq., sent me the following:—

"With regard to the elevation of the meteor of Feb. 11, I should think its altitude was 60° when I first saw it. I have taken the bearings of the road accurately by compass, and find that the meteor passed from N.N.W. to S.S.E†. I

* At my request Miss Faulkner had the distances measured from her eye (as she stood when she saw the meteor) to the parts of the walls over which she saw it appear and disappear, and the heights at the top of the walls above these parts. These distances are, from her eye to the western wall, 25·5 feet, and to the eastern wall 6 feet; the height of the top of the former wall above this horizontal line was 13·75 feet, and of the latter was 3·65 feet. Hence the altitude of the top of the wall, over which point she first saw the meteor, was 28° 20′, and therefore the height of the meteor when first seen must have exceeded fifty miles considerably. The altitude of the point on the wall behind which the meteor descended was 31° 19′; hence the height of the meteor at this time was less than thirty miles.

† The variation of the compass at Bedford is 22° 52′; hence the azimuth of meteor when first seen was 45½° W. of N., and when last seen was 45½° E. of S. At the time of sending this to press, I have not received any answer to my request, to have accurate measures taken.

can have no doubt as to the place it occupied when I saw it, as the point is determined by the space between two particular houses. The noise that I heard afterwards was within, I should certainly say, one minute after the explosion. Mr. Whitbread, with great reason, suspects that I was not in a fit state to determine this; but I was not really alarmed till I heard the report, which was very loud and continued, and which was the chief cause of terror, happening with the most brilliant starlight. I however was collected enough to observe my watch, and I was in a hurry to reach home. The distance I walked between the explosion and the first rumbling of the report was about 80 to 100 yards. I usually walk at the rate of four miles an hour, and according to this calculation the meteor when it exploded was about thirteen miles from me."

XXXIX. Rugby. Second communication from the Rev. H. Highton.

"The account which I gave you before of the meteor of Feb. 11th, was obtained by taking down the accounts of such persons who in different parts of the town were walking at that time, and afterwards going myself and stepping the distances, which were accurately given me, together with an estimate of the pace they were walking at.

"Since that, however, I have gone over the ground in company with each, making them point out as nearly as possible the points at which the meteor appeared to them to be. This more careful process leads to the following material alterations from my former account:—

"1. Though they told me that it passed straight over head, yet I find that was not the case, but that it passed from W. to E. at an angle of about 20° below the zenith, or 70° above the southern horizon.

"2. The time during which the light lasted was at least 50 seconds. The person who told me that he kept walking fast without stopping, through a space taking only 20 seconds to walk, must have been mistaken, and stopped on his way unconsciously to look at it. The duration, measured by the space walked over by the other person, was at least 50 seconds, and might have been 70 seconds.

"3. The point of the explosion was nearly due S.E.* (reckoning the magnetic and not the true meridian), and at an angle of about 20° from the horizon; so that it must have been at a very considerable distance from Rugby. This was what the narrator had called at first 'a few yards.'

* The variation of the compass at Rugby is 23° 15′; therefore the azimuth was 68° 15′ E. of S.

"4. The distance of time between the explosion and the detonation was at least 90 seconds, and probably between 90 seconds and 100 seconds.

"This may be considered as accurately ascertained. I have taken so great pains in getting the people to walk over the ground with me and point exactly where each circumstance took place, that you may consider these statements as fair approximations to the truth."

XL. Rugby. Third communication from the Rev. H. Highton.

"You may fully rely on it that the point of explosion was nearly due magnetic S.E.; the south end of needle standing at 0°, the reading of the circle where it is cut by the line joining the eye; centre of compass and the point of explosion of meteor is 45° or 135° from the west end of needle. The points observed by three observers are so plain and well-defined, that there is no room for more than a few degrees of error.

"The duration of light was certainly not less than 50 seconds; for a young man walking with his mother marked carefully, and by unmistakeable marks, the points where they were when the light first appeared, and again where they were when it exploded. If you were to see the places, you could perceive that they could not be mistaken; and the space could not be walked over by them in less than 50 seconds, though it might have taken more.

"The time between explosion and detonation is equally well marked as not less than 90 seconds and probably not more than 100 seconds.

"The direction may be more doubtful, but it was probably in a line running parallel with a line from west to east, at an altitude of about 70° above the S.S.W. horizon, as seen up a street running N.N.E. and S.S.W. If you come here I will show you the points, and you will see there can be no mistake about these points, which I tell you are clear."

XLI. Prestwood, Stourbridge, as copied from the *Times* of February 14th.

"Yesterday evening, at about half-past ten o'clock, I was observing the peculiar clearness of the atmosphere, the weather having rapidly changed from stormy rain, which had prevailed throughout the day, to that brilliancy of sky which usually betokens, at this season, frost; when suddenly I was almost bereft of sight by the appearance of a magnificent meteor, which made its appearance in a westerly direction, and passing rapidly across the zenith, exploded in the east at

about 15° above the horizon. It was followed by a luminous tail of extraordinary length and brilliancy, and the disc of its nucleus appeared to be nearly half the diameter of the moon when full. Fire-balls of a bluish colour fell from the tail in profusion.

"Some idea may be formed of the intense light cast from it, when I state that I could discern nothing whatever for several minutes after; indeed, many families in the neighbourhood, sitting in well-lighted rooms, where there were apertures or crevices through the shutters, were alarmed, and went out completely terrified. "G. Thompson, Jun."

XLII. Castle Donnington, lat. 52° 51′ 23″, and long. 1° 18′ 42″ W. of Greenwich. The Rev. Kirke Swann.

"At the time the meteor appeared I was in a room without any artificial light, my eyes directed to a window not obscured by curtains or blinds.

"The aspect 74° E. of true S. or E.S.E. 6° E. The sky was quite free from clouds in this aspect.

"The window was suddenly illuminated by a faint light of a pale bluish colour. The light became somewhat red, and rapidly increased to a brightness, which, from many considerations, is not over-estimated, if we say it was almost as bright as daylight when the sun is at the horizon.

"When the light had continued about 5 seconds (by a rough estimation) I drew near to the window. The meteor was then entering my field of view at the top of the window.

"Its motion in azimuth was too small to be perceived, but its altitude rapidly diminished; that is, its apparent motion was that of vertical descent.

"Its azimuth was from 35° to 40° E. of S.

"At the altitude of about 30° it divided into about six parts, all of small size, but of most intense brightness. These fell through about 5° of altitude (or rather more), and then instantly all was darkness.

"This was about 2 seconds after it entered my view at the top of the window, which makes the whole duration about 7 seconds by a very rough estimation. No sound was heard by any one here."

XLIII. Castle Donnington. Second communication.

"A person out of doors informs me that the whole sky was cloudless; but when the light was over, a bank of black clouds rose very rapidly from the horizon in N.W. to N.N.W., and covered the sky in a few minutes after the meteor. The wind at the time was S.W.

"It is necessary to say how these altitudes and azimuths are obtained.

"Placing myself nearly in the position I was in when I saw the meteor, I took the altitude and azimuth of the place I believe it to have occupied on the windows by instruments.

"The place of this meteor could not be taken, as is usual with meteors, by noticing what stars it was near, for its brightness obliterated all, even Jupiter. I estimated its position with respect to α Leonis, Jupiter and α Hydræ, by looking, directly it was dark again, in the right direction as nearly as I could; and I have also estimated it since by noticing the sun, moon and Jupiter when they have occupied nearly the same position on my window.

"It may be worth while to say what is the extent to which error in the position is probable.

"The meteor certainly entered my field of view at the altitude 40°, that of the top of the window; and its end was at the altitude of about 25°. It is quite certain it was more than 20°, and nearly certain that it was not more than 25°.

"The altitude which is most important is the bursting-point.

"30°* is the altitude at which the bodies were first perceived to be separate; there is little doubt here; the error could not exceed 3° each way.

"But as no change was perceived in the motion of the meteor at the time of bursting, either in velocity or direction, it is probable that it divided 2° or 3° higher than when it was perceived to consist of about six bodies.

"These bodies spread very little laterally. They appeared to divide, by the apparent motion of some being quicker than that of others.

"There can be little error in saying that no motion in azimuth was perceived, for the perpendicular divisions of the window would guide the eye.

"The azimuth is less important than the altitude; this is fortunate, since the probability of error in azimuth is greater than in altitude; for a little variation in the position of a body at 30° elevation makes a great variation in the azimuth, and also the certainty of a few inches in the place where I stood, affects the azimuth, but not the altitude. I think however it could not be at a greater distance from the true S. than 40° or less than 35°."

* The accounts from Castle Donnington were received shortly before this paper was prepared for the press, and therefore have scarcely influenced me in the following discussion of the results. The altitudes seem to be too high. I have requested them to be remeasured, but I have not received any further information at the time of sending the MSS. away.

XLIV. Near Chester. The Rev. Henry Linthwaite, B.A.

"As I was riding home on the night of Monday the 11th, about a quarter before eleven o'clock, with two friends, our attention was excited by the instantaneous appearance of a very bright meteor, which illuminated for a few seconds every object around us.

"Its altitude was about 60°, and it appeared to pass through a distance of 18°, as near as I could judge. Its position was nearly south, and its direction almost perpendicularly towards the earth, but slightly inclined eastwards. The colour of the head resembled that of the moon, but was brighter, and gradually deepened into a very beautiful rose-pink. It appeared to be extinguished or hidden behind a large cloud; no sparks from it were visible. The time of its path was not less than 4 or 5 seconds. I called on Dr. Moffatt, and communicated these particulars to him on Tuesday, the day following, and pointed out to him the exact position in the heavens where the meteor was visible."

This account was sent to me by Dr. Moffatt with his usual monthly meteorological report, and at my request the following measures were taken:—

"We have done our best to ascertain the altitude and azimuth of the meteor as it was seen by the Rev. Henry Linthwaite on the 11th of February at $10^h\ 45^m$ P.M. His memory, with regard to the locality of the meteor, has been assisted by a recollection of its relative position to certain stars in Orion, Auriga and Gemini; and as our observations have been made about a month after the appearance of the meteor, we did not omit taking into consideration the difference in the hour (about two hours earlier) of the culminating of these constellations.

"Mr. Linthwaite was seated sideways to the object when he first saw its light, so it must have travelled some distance before he saw the body; but at the time he first perceived it, it appears that it was equidistant from the foot of Auriga and the right arm and club of Orion, from which it pursued a course towards Gemini, where it was lost from view behind a large dark cloud which extended from N.E. to N.

"Its altitude at its first appearance was thus found to be 55° 36′; and when lost behind the cloud it was at an altitude of 42° 54′, and azimuth about 18° E. of S."

XLV. St. Cuthbert's College, Ushaw, Durham. Communicated in a letter to R. C. Carrington, Esq., of the Observatory at Durham, from the Rev. John Gillow.

"I shall be glad to furnish the meagre data that I have been able to collect respecting the meteor seen here on the

11th inst. It was seen by four of the Professors, viz. the Rev. Michael Gibson, the Rev. John Gibson, the Rev. Robert Gradwell, and the Rev. Chas. Gillow. These gentlemen were at the time entering the gates of the College Lodge in a closed carriage. The Rev. John Gibson was the first to perceive it, and called the attention of the others to 'a meteor.' It was at about a quarter before 11 P.M., was visible about 30 seconds; it left a train of light in its path, and terminated with an explosion, shooting forth brilliant coloured scintillations of red and purple in the manner of a rocket. This appearance made all conclude that it was a rocket sent up from Brancepeth.

"The night was very clear, but as they supposed it was only a rocket, they took no care to note its position by the stars. To obtain a rough calculation of its position, I have requested them separately to direct the telescope of the theodolite to the place where, as far as could be recollected, it seemed to each one. The Rev. M. Gibson directed it to $5° 17'$ altitude and $13° 15'$ azimuth, eastward of the south; the Rev. R. Gradwell to $5° 20'$ altitude and $5° 15'$ azimuth; and the Rev. John Gibson to $6° 48'$ altitude, and described its path as being from $5° 52'$ azimuth west to $5° 15'$ azimuth east, beginning at a lower altitude of about $2°$. This gentleman had the best view and saw it the longest, but it may have been visible a few seconds before it attracted his notice.

"It seems to me very remarkable, but all agree in saying that the scintillations after the explosion seemed to fall. I am of opinion that imagination must have aided this conception. Its whole appearance was very like that of a rocket, but of extraordinary brilliancy."

These are all the accounts I have received giving information upon this body, and I now proceed to discuss them, with the view of determining its distance, path, velocity, &c.

1. *Determination of the spot over which the Meteor was vertical on its first appearance.*

The observations available for this determination are those of J. Hannane, Esq., at Yeovil; of Miss Faulkner at Deddington; and of the Rev. H. Linthwaite at Chester. Of these observers, the most favourably situated was Mr. Hannane, who at the time was looking at that part of the sky where it appeared, searching for the pole-star, and saw the meteor burst out in a position $8°$ or $9°$ W. of N. Miss Faulkner was well situated, being in a small garden, and looking at the time towards that part of the heavens whose azimuth was

62° W. of N. Mr. Linthwaite was riding in the direction of E. and W., and did not see its first appearance, but its change of azimuth at this moment was small, and the interval of time was so short, that the estimated azimuth (nearly S.) must be near the truth. These lines intersect each other very nearly over a spot at about thirteen miles east of Montgomery. This meteor, therefore, seems to have been vertical at this spot, or nearly so, and to have been first seen there in a state of ignition.

2. *Determination of the Path of the Meteor.*

From this spot it proceeded over Shropshire and Warwickshire, including the southern boundary of Staffordshire, from thence over Northamptonshire to Bedfordshire. In its course it passed the zenith of Stourbridge nearly, a little south of Coventry, north of Warwick, and south of Northampton, Rugby and Bedford.

3. *Determination of the spot over which the Meteor was vertical at its explosion.*

The observations available for this determination are the following:—

At Brighton its azimuth was		5° W. of N.
At Greenwich	...	19 W. of N.
At Deddington	...	82 E. of N.
At Rugby	...	68 E. of S.
At Castle Donnington	...	40 E. of S.*

The intersection of these lines is at a spot very near to Biggleswade, and I have finally decided that the most probable place over which the meteor was at its explosion is situated at about $1\frac{1}{2}$ mile from Biggleswade, and bearing 18° S. of E.

4. *Determination of the distance of the Meteor from the earth on its first appearance.*

The data for this determination are the following:—

At Yeovil its altitude was 36°, as measured by a theodolite. At Chester its estimated altitude at the time of observation was 60°, after the lapse of a month it was determined by measurement to be 55° 36′. The distance from Yeovil is 117 miles, and from Chester is $46\frac{1}{2}$ miles. Hence, from the observation at Yeovil, the distance from the earth was 88

* In account XXII. at Hull it is stated that the meteor exploded about 15° W. of the meridian. As the right ascension of 15 Argus is $8^h\ 1^m$, and the sidereal time was $8^h\ 9^m$, the star had passed the meridian of Hull when the observation was made by a few minutes only, therefore its azimuth was very small.

miles; from the Chester observation, with the first estimated altitude, it was 82 miles, and from the measured altitude it was 76 miles. I think the observation at Yeovil is entitled to double the weight of that at Chester, particularly as the latter observer was riding in the direction of east and west, and he had to turn his head to see the meteor. The meteor, therefore, at the time of its first appearance, was at about the distance of 84 miles from the earth.

5. *Determination of the Distances of the Meteor from the earth at different parts in its path.*

At Chester, the Rev. H. Linthwaite lost sight of the meteor at a point 18° E. of S., and at an altitude of 42° 54′.

This azimuth intersects the line of direction of the meteor at a point about 8 miles west of Kidderminster, and at the distance of 51 miles from Chester. Hence its distance from the earth at this time was 48 miles.

At Yeovil, the observer lost sight of the meteor behind a chimney, whose azimuth was 11° 40′ E. of N. This azimuth intersects the line of direction at a point 7 miles E. by N. nearly from Stourbridge, and at a distance of 99 miles from Yeovil. Hence its distance at this time was 42 miles*.

At Reading, the observer first saw the meteor between 5° and 8° W. of N. Assuming that this azimuth met the line of direction at a point about 5 miles S.W. of Northampton, at the distance of about $53\frac{1}{2}$ miles from Reading, with the estimated angle of 26°, its height at this time was 25 miles.

At Langport, the observer lost sight of the meteor when it was at an altitude of 10°, bearing N.E. This azimuth intersects the line of direction about 7 miles W. of Bedford, and distant from Langport 111 miles. Hence the height of the meteor at this time was 23 miles.

6. *Determination of the Height of the Meteor at the time of its explosion.*

The data we have for this determination are the following:—

At Euston Square, distance 40 miles, the altitude measured was 23°.

* At Hampstead Road, in account XII., the observer saw the meteor shortly after its commencement, a little above the Pleïades, the altitude of which was 32°. Assuming the altitude of the meteor at this time as 33°, its distance from the earth was 60 miles.

At the same place the meteor disappeared near the lower part of the constellation of Cassiopœia, or above an altitude of 25°, and azimuth 36° W. of N., hence its height at this time was about 24 miles.

In account X., Mr. Hind observed the meteor pass from below α Persei to α Cassiopœia. Assuming its altitudes at these times to be $37\frac{1}{2}$° and $28\frac{1}{2}$° respectively, with azimuths 60°, and 36° W. of N., the heights were 83 miles and 28 miles respectively.

At Reading, distance $53\frac{1}{2}$ miles, the altitude measured was 16°, or rather more.

At Rugby, distance $48\frac{3}{4}$ miles, the altitude measured was 20°.

At Carrington, distance 73 miles, the altitude measured was 13°.

At Hull, distance $118\frac{1}{2}$ miles, the altitude estimated was 12°.

At Prestwood, distance 80 miles, the altitude estimated was 15°.

At Durham, distance 197 miles, the altitude estimated was $5\frac{3}{4}$°.

Hence, from the observations at—

Euston Square its height was		17	miles.
Reading	...	16	...
Rugby	...	18	...
Carrington	...	17	...
Hull	...	27	...
Prestwood	...	25	...
Durham	...	21	...

Giving double weights to the results from the measured angles, the resulting mean value is 19 miles as the distance of the meteor from the earth at the time of its explosion.

7. *Determination of the height of the parts of the Meteor while luminous after explosion.*

The data we have for this determination are the observations of the Astronomer Royal at Greenwich, and those of the Rev. J. Wharton at Brighton.

At Greenwich, distance 44 miles, the altitude of the bodies was 14° 53′.

At Brighton, distance $88\frac{1}{2}$ miles, the altitude of the bodies was 7°.

Hence the height of the luminous bodies at this time from both these observations was 10 miles.

8. *Determination of the real size of the Meteor at the time of its explosion.*

I cannot pretend to determine this point with any great precision, it having been so differently represented by the different observers. The data we have for this determination are the following:—

1st. I shall confine myself to the description of those observers who were situated within or about 50 miles of the place of explosion.

At Hampstead Road it was about four times as large as Venus.

Near Hartwell it was as large as the full moon.

At Deddington its appearance at first was much larger than Jupiter, and when lost sight of its diameter was greater than that of the moon.

2nd. The following descriptions are by observers who were situated between 50 and 100 miles of the place of the explosion:—

At Prestwood its disc appeared to be about one-half the size of the moon.

At Bath, a ball 8 or 9 inches in diameter.

At Birmingham, a column 2 feet in width and 20 feet in length.

At Carrington, an elongated luminous ball, smaller than the moon.

At Blakeney, Norfolk, it was 20 feet in length.

At Southampton, a full-sized orange.

3rd. At places exceeding the distance of 100 miles:—

At Yeovil, two or three times the size of Mars.

At Langport, equal to Venus when brightest.

At Hull, one-half the size of the moon.

At Sidmouth, a bar of light a yard in length.

At Bristol, a ball of fire.

At Penzance, an immense globular expanse of flame.

There is little doubt that its dazzling brightness has caused some deception, and as imagination may have helped to enlarge the object, it perhaps will be better to take the smaller estimations generally. It seems, however, that at the distance of 50 miles it was compared in apparent size to that of the full moon; had it been so large, its diameter would have been about 2500 feet.

In the second set of estimations it was compared to a ball 8 or 9 inches in diameter; to a ball smaller than the moon; to a disc about one-half the size of the moon; and to a full-sized orange; and in the third set to one-half the size of the moon. It is difficult to understand so as to make use of some of the comparisons; it is exceedingly doubtful to what size to refer a ball 8 or 9 inches in diameter; the full moon appears to different persons of different sizes; some will say she is 6 inches in diameter, and others will say she equals 12 inches. Let us give some allowance for imagination, and the best result at which I can arrive is, that at about 100 miles distance the meteor subtended an angle of about 12′ of arc, and if so, its real diameter was about 1800 feet. Where the accounts are so vague, the most moderate calculations are most likely to be true; I therefore consider the diameter of the body to have been from 1800 feet to 2000 feet.

9. *On the Report at the explosion of the Meteor.*

At Bromham, near Bedford, the noise of the detonation was described as a roar, and it was heard by everybody, both by those who saw the meteor as well as by those who did not.

At Cardington, near Bedford, Mrs. Whitbread, in a letter written before hearing of the meteor, says, "that at a quarter before 11 o'clock she heard a tremendous rushing, rumbling noise, that all the servants heard it, and some saw a great glare of light."

At Enstone a dreadful explosion was heard by everybody. At Hartwell there was a report like thunder.

At Rugby the noise was that like the crash of a falling building.

At Deddington the noise was heard as the report of a cannon at a great distance. At Banbury, a report as loud as thunder.

The sound at the time of explosion must have been very great to have been heard at places exceeding 50 miles, and compared there to loud thunder, &c.

10. *On the estimated interval of time between the explosion and hearing the report.*

At Bedford about 1^m; at Enstone about 2^m; at Hartwell 2^m or 3^m; at Rugby 90^s to 100^s; at Bromham 1^m to 2^m; and at Deddington 4^m to 5^m. These values are discordant.

11. *On the Brilliancy of the Light of the Meteor.*

The extreme brilliancy of the light is mentioned in every account, from Penzance to Durham. Usually it was described as intensely brilliant; and at places near whose zenith it passed, it was most brilliant. Some persons compared it to strong sunlight; and even at places where the sky was covered with black clouds, as at Blakeney, the light was described as being so brilliant as to fully illuminate every object. Indeed its brilliancy may be judged from the fact, that the observer at Raby Castle (at the distance of 180 miles) at first thought that some part of the building was on fire.

12. *On the Colour of the Meteor.*

At Brighton, Uckfield, Langport, Enstone, and at Hull, it was described as bluish; at Yeovil it was first red like Mars, then changed to a brilliant white light. At Bath it was first red and then blue. At Greenwich it was of a strong yellow colour; and at Lewisham, as observed by Mrs. Glaisher, it was of a yellow colour. At Hampstead Road it appeared as

of a dull golden lustre; at Hampstead it was first white, then yellow, and then pink. At Birmingham it was described as of the gorgeous colours of the Iris. At Carrington, pink and white; at Chester, first a pale yellow, or of silvery brightness, then deepened into a beautiful rose-pink; and at Reading of a dull red.

The differences of colour are doubtless attributable to the circumstances under which the observer has viewed the meteor: in some cases marshy land or a river has intervened, and in others the meteor may have been seen from high land with a clear atmosphere.

13. *The Time of Visibility of the Meteor.*

The observer at Chester lost sight of the meteor in 4^s or 5^s, it being at this time over a place a few miles west of the zenith of Kidderminster. The observer at Yeovil saw it from its commencement till he lost sight of it when in the zenith of a place a few miles east of Stourbridge, after an interval of 7^s or 8^s. These two accounts are accordant. The observer at Langport saw the meteor at about the time the observer at Yeovil lost sight of it, and it disappeared from his view in 3 seconds, when the meteor was a few miles west of Bedford, or at about 1^s in time before its explosion. These times together make up an interval of about 11 seconds, from the time of its first appearance to the time of its explosion.

The observer at Southampton saw it for 2^s. At Euston Square the time of duration was estimated as 2^s or 3^s. At Enstone a permanence of light of several seconds was noticed. At Hampstead Road the time it was visible was 3^s or 4^s. At Bromham the light continued full half a minute. At Rugby, by account XVII., the duration of light was 20 seconds; and by account XXXIX. it was 50 seconds. At Wolverhampton, the time, as near as could be judged, was 60 seconds; at Carrington it was visible 3^s or 4^s; at Raby Castle 6^s; at Brixworth half a minute, as furnished by the Rev. C. F. Watkins, the Vicar. At Durham it was visible 30 seconds; and at Deddington the time was estimated as 40 seconds. These intervals of time are very discordant, and in some cases imagination must totally have misled the observers.

14. *On the Velocity of the Meteor.*

In regard to its velocity it was certainly great; in judging of it, we may neglect entirely the diurnal motion of the earth, and if we suppose for a moment, that the earth did not move in its orbit during the time of its continuance, estimated at 11 seconds, then it passed fully at the rate of 15 miles in one

second, but as the meteor was moving in the order of planetary motion nearly, this motion has to be increased by that of the orbital motion of the earth, which makes the real velocity of the meteor to have been greater than 30 miles in one second, which seems almost incredible.

15. *On the Absolute Times of its Appearance.*

We are totally dependent for our knowledge of this upon the observations of the Astronomer Royal. He saw the meteor after explosion at $10^h\ 41^m\ 28^s$ Greenwich mean solar time; and allowing 12^s or 13^s for the interval of time from its commencement to the time of observation of the Astronomer Royal, we have $10^h\ 41^m\ 16^s \pm$ Greenwich mean time for its commencement, and $10^h\ 41^m\ 27^s \pm$ for the time of its explosion.

16. *On the Tail or Train of Light.*

This was variously described by the different observers. At Southampton it was mentioned as a luminous appearance; at Yeovil, as a tail; at Langport, a pale blue train; at Euston Square, a compact sheet of flame; at Hampstead Road, a waving blade of red flame; at Hampstead, a long tail; at Rugby, an elongated luminous ball; at Carrington, head and tail $2\frac{1}{2}°$ in length; at Deddington, a brilliant tail of light; at Bristol, a tail 15° in length; at Reading, the breadth of Venus extended into a line; at Prestwood, a luminous tail of extraordinary length and brilliancy. In a recent letter I have received from the Rev. J. Jordan, he says that Mr. Kimber was most favourably situated, there not being a single obstacle of any kind; and he says there was certainly no tail, but merely the trail of light depicting its path.

It seems, however, certain, both from the general accounts, and from sketches which have been furnished to me of its appearance, that there was a stream of light of great brilliancy, of many miles in length.

17. *On the Curve described by the Meteor, and concluding Remarks.*

During the first part of the progress of this meteor it very rapidly descended obliquely towards the earth, in such manner, that at first it was between 80 and 90 miles from the earth, and less than 50 miles distant within 4 seconds or 5 seconds afterwards, the two places over which it was vertical at these times being separated by about 17 miles; it then decreased less rapidly till—

When over a place about 37 miles from the first-mentioned place, its distance from the earth was 42 miles.

When over a place about 80 miles from the first-mentioned place, its distance from the earth was 25 miles.

When over a place about 90 miles from the first-mentioned place, its distance from the earth was 23 miles.

When over a place about 110 miles from the first-mentioned place, its distance from the earth was 19 miles, when it exploded.

The curve described by the meteor was that of the parabola, as will be seen by laying the above numbers, or their complements, to 84 miles, upon a line of abscissa.

After the explosion, the luminous bodies were seen till they were within 10 miles of the earth. The report accompanying the explosion was so great, that I am inclined to believe that the substance of the meteor was of a firm texture, broken into many pieces by the extraordinary expansion of an elastic fluid; if so, its particles would fly off in all directions; some would describe parabolic curves, as mentioned by the Rev. C. J. Goodhart; some would continue to move with accelerated force in the same direction, and some would fall vertically. It seems probable that some parts of this body may have reached the ground within a few miles round Biggleswade. It seems certain that this meteor must have come from the regions of space far beyond the influence of our vapours; and this fact, together with its extreme velocity, and the intensity of the light, are circumstances more conformable to a solid than to a gaseous substance.

[The original accounts will be preserved in the Archives of the Royal Observatory, Greenwich.]

XXXIII. *On the Nitroprussides, a New Class of Salts.* *By* Dr. Lyon Playfair, *F.R.S., F.C.S.*

[Continued from p. 221.]

Section III.—*Changes experienced by certain Nitroprussides when their solutions are heated or kept.*

18. SEVERAL of the nitroprussides, especially nitroprussic acid, nitroprussides of ammonium, barium and calcium, deposit either prussian blue or oxide of iron when their solutions are heated or are kept for some time. The residual liquid, after evaporation, yields crystals of the same shape and exactly of the same properties as before. Analysis however

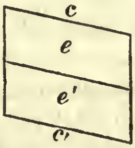
shows that some change has resulted in their composition, for the iron or electro-negative metal is now in greater than atomic proportion to the electro-positive metal. The proportion of carbon is also somewhat different. Still the difference in composition is not very considerable, although decidedly marked; it is not however sufficient to cause any obvious alteration in their general properties. In fact there is an attached impurity, probably a cyanide of iron, which cannot now be removed by crystallization, precipitation, digestion with nitric acid, or any of the ordinary means of purification. This impurity, if it be one, remains so obstinately attached that all methods of purification have quite failed to remove it. This circumstance, before it was understood, had thrown the greatest difficulties in the way of the inquiry, and protracted it to a most tedious length by preventing the attainment of accordant results. It is to prevent the like inconvenience to those who repeat these experiments that this section of the paper is specially devoted. Attention has previously been drawn to the fact, that the nitroprussides form chemical compounds with the cyanides of iron. This seems to be a case of the same kind, but of more ultimate union. The impurity or chemically attached cyanide in this case appears to be $FeCy^2$, or perhaps $FeCy + H\,Cy$, judging from analysis only, for its separation has not been accomplished. The proportion in which it is present is very small, generally only $2(FeCy^2)$ to 7 equivs. of a nitroprusside, or if it be a chemical compound, $7(Fe^5\,Cy^{12}\,3NO + 5R) + Fe^2\,Cy^4$. Still as the crystalline form and all the properties of the nitroprussides remain unchanged, we can scarcely view its presence in any other light than as an impurity. Several of the nitroprussides, viz. nitroprussic acid and the nitroprussides of ammonium and calcium, have not yet been obtained free from this impurity, and are therefore described in this section.

Nitroprussic Acid.

19. The mode of preparation of this acid has been already described at page 209. It is however most readily prepared from nitroprusside of silver by adding to it as much hydrochloric acid as suffices to form chloride of silver with the silver in the salt. The dark red solution thus obtained soon evolves hydrocyanic acid, even in the cold, and after a time prusside of potassium indicates the presence of iron in solution. If the solution be heated, it deposits abundance of a brown precipitate resembling oxide of iron. When the latter is separated by filtration, and the solution is evaporated *in vacuo* over sul-

phuric acid, crystals are formed and may be separated; they must be dried over sulphuric acid, as they are exceedingly deliquescent. These crystals belong to the oblique system, but on account of their excessive tendency to deliquesce, it is difficult to measure their angles with accordant results. The angles between normals to the only faces which gave results to be depended on, are stated by Prof. Miller to be as follows:—

	°	′
ec	36	57
e′c′	36	57
ee′	106	6



It will be seen that the equality of the angles *ec* and *e′c′* is a tolerably certain indication that the crystals belong to the oblique system.

The acid made by the action of hydrochloric acid on nitroprusside of siver, and evaporated over sulphuric acid in the cold, crystallized (light being excluded) without the deposition of oxide of iron, but the smell of hydrocyanic acid, accompanied by a peculiar pungent smell, was strongly perceptible. Analysis shows that these crystals are the same as those obtained from a boiled solution.

Properties of the Crystallized Acid.—The crystallized acid is of a dark red colour, and has a very acid reaction, the crystals being generally flattened and of tolerable size. They are quite as deliquescent as chloride of calcium. They dissolve to a large extent in water, and are also soluble in alcohol and in æther. They may be dried in the water-bath without change, but their aqueous solution cannot be boiled without decomposition.

The following analyses were made on crystals obtained from a boiled solution, and were dried at 212°. The acid was that made by the action of hydrochloric acid on the silver salt. Nos. I. II. and III. were preparations made at distinct times.

The iron was determined by calcination and by treating the residual oxide with nitrate of ammonia.

I. 2·345 grs. gave 0·800 gr. peroxide of iron.
II. 3·915 grs. gave 1·325 gr. peroxide of iron.
III. 3·580 grs. gave 1·220 gr. peroxide of iron.

The combustions were made in the usual way.

I. 7·720 grs. gave 7·005 grs. CO^2 and 1·175 gr. HO.
II. 10·810 grs. gave 9·880 grs. CO^2 and 1·665 gr. HO.
III. 4·385 grs. gave 3·980 grs. CO^2 and 0·700 gr. HO.

An estimation of nitrogen by Bunsen's method gave the following result:—

	Obs. vol.	Barom. inches.	Therm. °	Col. Merc.
Vol. of mixed gases (moist)	89·5	29·994	7·0 C.	152·7
Vol. after absorption (dry)	37·4	30·015	9·2 C.	205·2

Corrected vol. of mixed gases . . .	52·995
After absorption of carbonic acid . .	20·570
Nitrogen . . .	32·425

Hence the proportion of nitrogen to carbonic acid is 1 : 1·576.

	I.	II.	III.			Calculated.
Iron . . .	23·88	23·69	23·85	5	140	24·26
Carbon . .	24·74	24·92	24·75	24	144	24·95
Hydrogen .	1·69	1·71	1·77	11	11	1·90
Nitrogen .	36·73	36·73	36·73	15	210	36·39
Oxygen .	12·96	12·95	12·90	9	72	12·50
	100·00	100·00	100·00		577	100·00

The calculated result, especially as regards the hydrogen, is not sufficiently close to be the true expression of the analysis, but it is here given to show how far the acid differs from pure nitroprussic acid. It is indeed probable that the acid dried at 212° only contains 10 equivs. of water.

The acid is so remarkably deliquescent that it is very difficult to ascertain how much the crystals lose in the water-bath. The following analysis of the salt dried *in vacuo* over sulphuric acid shows a higher state of hydration. The sample analysed had never been heated, even in solution, so that it evaporated without the deposition of oxide of iron. Still the oxide was detected in the mother-liquor by ferrocyanide of potassium.

I. 3·225 grs. gave 1·010 gr. peroxide of iron.

II. 3·235 grs. gave 1·020 gr. peroxide of iron.

I. 5·830 grs. gave 5·020 grs. carbonic acid and 1·09 gr. water.

II. 8·225 grs. gave 7·060 grs. carbonic acid and 1·51 gr. water.

	I.	II.	Mean.
Iron . . .	21·92	22·07	21·99
Carbon . .	23·48	23·32	23·40
Hydrogen .	2·07	2·03	2·05
Nitrogen . . } Oxygen . . }	52·53	52·58	52·56
	100·00	100·00	100·00

A silver salt made from the well-crystallized acid showed that the iron was in excess, and that the carbon was in the usual proportion (see pp. 280, 281). The analyses of these silver salts are given further on, in order to avoid repetition.

The discussion as to the constitution of the acid is also deferred to that place.

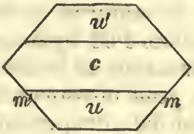
Nitroprusside of Ammonium.

20. When ammonia is added to an excess of nitroprusside of iron the latter is decomposed, oxide of iron being precipitated, but during the action nitrogen gas is evolved. If the red-coloured solution caused by filtration be evaporated in the air-pump, a difficultly crystallizable salt is obtained, which very readily decomposes, turning blue in the water-bath, and even when dried over sulphuric acid *in vacuo*. This salt is probably the true nitroprusside of ammonium, but it has not been obtained pure for analysis. If a solution of this salt be heated, prussian blue is deposited, and the filtered dark-red liquid, being evaporated by a gentle heat, now crystallizes in a warm place very readily, and in fine large red crystals, which are so dark as to be almost of a black colour. These have been measured by Prof. Miller; they are prismatic, but the angles given are only approximative, the faces of the crystal examined being imperfect.

Symbols:—*c* 001, *m* 110, *u* 011.

Angles between normals to the faces:—

	°	'
mc	90	0
mm'	88	4
uc	55	3
uu'	110	6



They are twin crystals, the twin faces being *m*.

This salt is very soluble in water, from which it is not precipitated by alcohol. It is very slightly deliquescent. The salt dried in air loses water in the water-bath.

18·648 grs. lost at 212° 2·928 grs., or 15·701 per cent.
10·915 grs. lost at 212° 1·800 gr., or 16·491 per cent.
11·502 grs. lost at 212° 1·948 gr., or 16·936 per cent.
45·400 grs. lost at 212° 6·850 grs., or 15·088 per cent.

16·054

The iron was determined by calcination.

I. 10·905 grs. gave 3·455 grs. peroxide of iron.
II. 12·954 grs. gave 4·070 grs. peroxide of iron.

The combustions made with chromate of lead gave the following results:—

I. 9·822 grs. gave 2·903 grs. HO and 8·251 grs. CO^2.
II. 12·765 grs. gave 3·682 grs. HO and 10·494 grs. CO^2.
III. 7·215 grs. gave 2·010 grs. HO and 6·020 grs. CO^2.

The nitrogen was determined by Dumas' quantitative method.

I. 4·494 grs. salt gave 112 C.C. gas, the therm. being 47°½ Fahr., barom. 29·844 in.

II. 3·372 grs. salt gave 83 C.C. gas, the therm. being 50° Fahr., barom. 29·550 in.

This, calculated on 22·7 per cent. carbon, gives 43·619 per cent. nitrogen.

Again, 8·747 grs. salt distilled with a weak solution of soda, gave a distillate which, collected in hydrochloric acid, yielded 15·021 grs. platinum salt.

	I.	II.	III.	Mean.
Iron	22·177	21·993	...	22·085
Carbon . . .	22·901	22·420	22·755	22·692
Hydrogen . .	3·283	3·204	3·095	3·194
Nitrogen . . .	46·894	45·076	...	45·985
Oxygen . . .	4·745	7·307	...	6·044
	100·000	100·000		100·000

The ammonium per cent. from the amount of platinum salt is 13·872.

It is obvious that there is little hydrogen as water, for the greatest part is required to make up the ammonium (13·872 per cent. requires 3·08 hydrogen). Reserving, as in the other cases, the discussion as to the cause of difference between this salt and the prue nitroprusside, it will be convenient to give the calculation for nitroprusside of ammonium, of which the formula would be $Fe^5 Cy^{12} 3NO, 5NH^4 + 2HO$.

5	Iron	140	22·36
24	Carbon . . .	144	23·00
20	Nitrogen . .	280	44·72
22	Hydrogen . .	22	3·51
5	Oxygen . .	40	6·41
		626	100·00

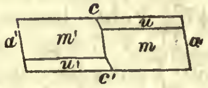
The hydrogen, but not the other constituents, would agree better with the above formula minus 2 equivs. of water; the hydrogen by the latter would be 3·28 per cent.

Nitroprusside of Calcium.

21. To prepare this salt, nitroprusside of iron or of copper is decomposed by milk of lime, the nitroprusside being kept in decided excess. A dark red solution is obtained, which on evaporation, even at a gentle heat, deposits prussian blue. When sufficiently concentrated the solution yields crystals of a dark red colour, and of considerable lustre. The crystals

belong to the oblique system. They have been approximatively measured by Prof. Miller.

Symbols:—*a* 100, *c* 001, *m* 110; there are besides one or two faces in the zone *c m c'*, the symbols of which have not been found.



Cleavage *a* very perfect.

Angles between normals to faces approximately:—

	°	′
ac	82	0
ma	70	0
mm'	40	0

The values of *cu* were extremely discordant. In the best crystals, the angle between normals to *cu* was found to be 71° 41′.

Nitroprusside of calcium is very soluble in water, and in its behaviour to reagents is exactly the same as the soluble nitroprussides already described. By the mean of two experiments the crystallized salt lost 17·85 per cent. of water in the water-bath at 212°.

The salt was analysed by fusion with nitrate of ammonia, the iron and lime being determined in the usual way.

13·29 grs. gave 4·004 grs. peroxide of iron and 4·698 grs. carbonate of lime.

8·33 grs. burned with chromate of lead gave 6·56 grs. carbonic acid and 0·82 water.

				Calculated.
Iron	21·09	5	140	21·11
Calcium	14·14	5	100	15·08
Carbon	21·47	24	144	21·71
Hydrogen	1·09	5	5	0·75
Nitrogen, Oxygen	42·21	15, 8	210, 64	41·35
	100·00		663	100·00

It will be seen that this salt belongs to the class which has dissolved some of the cyanide of iron resulting from its partial decomposition, and that therefore the electro-positive metal is in too small quantity. Allowing for this impurity, which cannot be removed, it is probable that the pure nitroprusside of calcium has the formula $Fe^5\,Cy^{12}\,3NO,\,Ca^5+5HO$. The loss of water in the water-bath corresponds to 15 equivs., which ought to have given the loss as 17 per cent. In one experiment it lost 17·44 per cent., in another 18·26. We may conclude that the formula of the crystallized salt is $Fe^5\,Cy^{12}\,3NO,\,Ca^5+20HO$.

Altered Nitroprusside of Barium.

22. When a solution of nitroprusside of barium is boiled, it deposits a brown precipitate containing both iron and barium*. The solution now crystallizes either in pyramidal or in prismatic crystals, that is, in the first state when crystallized slowly, in the second when deposited quickly from a hot solution. It is now found that the salt is inconstant in composition, different preparations giving very discordant results. The salt is however peculiarly difficult to dry, having to be kept in the water-bath for days before it ceases to lose weight; it abstracts water when dried most speedily from the atmosphere.

It is found that the carbon is increased in a marked degree. The following two specimens were made at different times and analysed. Analyses I. and II. were made on the same specimen, but crystallized over again for analysis II. No. III. is on a totally different specimen.

I. 14·40 grs. gave 8·62 grs. BaO, SO^3 and 3·12 grs. $Fe^2 O^3$.
II. 15·90 grs. gave 10·17 grs. BaO, SO^3 and 3·68 grs. $Fe^2 O^3$.
III. 14·135 grs. gave 8·47 grs. BaO, SO^3 and 3·06 grs. $Fe^2 O^3$.

The combustions were made with chromate of lead.

I. 11·735 grs. gave 7·730 grs. CO^2 and 1·390 gr. HO.
II. 10·610 grs. gave 7·145 grs. CO^2 and 0·700 gr. HO.
III. 14·045 grs. gave 8·800 grs. CO^2 and 1·900 gr. HO.

	I. 1st Crystallization.	II. 2nd Crystallization.	III. New portion.
Iron	15·16	16·27	14·76
Barium	35·57	37·59	37·85
Carbon	17·96	18·34	17·08
Hydrogen . .	1·31	0·73	1·50

But a new portion of barytes salt did not give the same result; the portion analysed was in prismatic crystals, and crystallized twice.

I. 11·65 grs. gave 6·58 grs. BaO, SO^3 and 2·49 grs. $Fe^2 O^3$.
II. 17·22 grs. gave 9·83 grs. BaO, SO^3 and 3·58 grs. $Fe^2 O^3$.

I. 6·87 grs. gave 3·87 grs. CO^2 and 0·52 gr. HO.
II. 13·62 grs. gave 7·44 grs. CO^2 and 0·69 gr. HO.

* The barytes used in decomposing the nitroprusside of copper was that made by boiling peroxide of manganese with sulphuret of barium. It always contains a little hyposulphite, and the brown precipitate was found to contain sulphate of barytes.

	IV. 1st Crystallization.	V. 2nd Crystallization.
Iron	14·96	14·55
Barium . . .	33·23	33·60
Carbon . . .	15·41	16·38
Hydrogen . .	0·83	0·55

Another portion, in flat prismatic crystals, made by neutralizing nitroprussic acid with carbonate of barytes, gave the following results:—

12·33 grs. gave 6·61 grs. sulphate of barytes and 2·42 grs. peroxide of iron.

6·60 grs. gave 4·005 grs. carbonic acid and 1·040 gr. water.

	VI.
Iron	13·73
Barium	31·53
Carbon	16·52
Hydrogen	1·75

In this case the salt lost no more in the water-bath, although this was to have been expected from its larger quantity of hydrogen.

In all these cases the specimens were excellently crystallized, and yet there is a greater or less quantity of a foreign substance prevailing in all, and producing results so very discordant. In the first two portions analysed the barium is to the carbon (37·01 : 17·79) almost exactly as 1 equiv. : $5\frac{1}{2}$ equivs., and the iron is to the carbon, sensibly though not so exactly, in the same proportion. In analysis VI., the iron is to the carbon as 28 : 33·7, or rather more than 1 : $5\frac{1}{2}$, while the barium is to the carbon as 1 : 6. Again, in analyses IV. and V., the iron is to the carbon as 1 : 5, and the barium to the same element 1 : $5\frac{1}{2}$.

Finally, it will be seen further on that the silver salt made from these altered salts of barium do not contain this excess of carbon. The filtrate from the silver salts yields on evaporation and incineration a small quantity of a black ash, but the quantity being so small the nature of the substance could not be ascertained. We can scarcely suppose that it is a ferrocyanide, because we should have expected to have it precipitated by nitrate of silver, even though it could not be recognized by its usual tests. It would be useless without further information to speculate upon the probable nature of the impurity. Sufficient however has been shown to prove that the most complicated results may attend the analysis of specimens of nitroprusside of barium prepared from solutions which have been heated and thus partially decomposed.

Altered Nitroprusside of Sodium.

23. The previous analyses of the crystallized nitroprussic acid and of the nitroprussides of ammonium and barium, and the composition of the silver salts prepared from them, show a want of accordance between the iron in the electro-negative constituent and the metal in the electro-positive one. The iron in all these cases is about a half per cent. in excess, therefore not sufficient to be considered as being in atomic proportion. It was thought, from the very distinct crystallization of the sodium salt, that this excess might not accompany it if prepared from the respective silver salts of the above compounds. Accordingly the silver salt was decomposed by an equivalent quantity of hydrochloric acid. The resulting solution was neutralized with carbonate of soda and crystallized. Analyses I. and II. were made on a salt thus prepared from crystallized nitroprusside of barium. Analysis III. on a salt similarly made from nitroprusside of ammonium. Again, when we referred to the action of caustic soda on the nitroprussides, it was obvious that by using a less quantity of the alkali than sufficed to effect the complete decomposition, a nitroprusside with a similar impurity in solution was to be expected.

Analysis IV. was made on a specimen thus prepared, and its accuracy is confirmed by a future analysis of a silver salt.

I. 13·695 grs. gave 3·72 grs. peroxide of iron.
II. 20·93 grs. gave 5·72 $Fe^2 O^3$ and 9·93 NaO, SO^3.
III. 15·35 grs. gave 4·25 $Fe^2 O^3$ and 7·10 NaO, SO^3.
IV. 11·13 grs. gave 3·07 $Fe^2 O^3$ and 5·06 NaO, SO^3.

The combustions were made with chromate of lead.

II. 13·34 grs. gave 9·74 grs. CO^2 and 1·58 gr. HO.
III. 14·475 grs. gave 10·68 grs. CO^2 and 1·67 gr. HO.
IV. 6·730 grs. gave 5·33 grs. CO^2 and 1·01 gr. HO.

	From barium salt.		From ammonium salt.	By action of caustic soda.
	I.	II.	III.	IV.
Iron	19·00	19·12	19·38	19·30
Sodium	...	15·37	15·00	14·72
Carbon	...	19·91	20·12	21·59
Hydrogen	...	1·31	1·21	1·65
Nitrogen, Oxygen	...	44·39	44·29	42·74
		100·00	100·00	100·00

It will be seen from these analyses that the excess of iron still remains, and this is further confirmed by silver salts again made from them and analysed. It will also be observed that

in specimen IV. we have the same remarkable increase in carbon as observed in the barium salt; the sodium is to the carbon as 1 : $5\frac{1}{2}$, which is exactly the proportion found in the latter salt; but this excess of carbon does not go down with a silver salt made from it.

Examination of the Silver Salts made from the altered Nitroprussides.

24. To save unnecessary repetition, the numerous analyses made of the silver salts are here brought together, although it might have been more distinct to have introduced them under the respective salts from which they were made. The reason for converting them into silver salts was, that from the high atomic weight of silver and its accuracy of determination, the atomic accordance or disagreement between it and the iron could more readily be perceived.

Analyses I. II. and III. were made on three different preparations of silver salt made from three different specimens of crystallized nitroprussic acid, by adding the latter to nitrate of silver.

Analysis IV. was made upon a portion of II. treated on sand-bath with strong nitric acid in the hope of dissolving out the excess of iron. A very small quantity of iron was detected in solution by prusside of potassium.

Analysis V. was made on the silver salt prepared from crystallized nitroprusside of ammonium.

Analyses VI. and VII. from silver salt precipitated from crystallized nitroprusside of barium, which contained 17·96 grs. of carbon, or in which the barium was to the carbon as 1 : $5\frac{1}{2}$.

Analysis VIII. On previous silver salt digested on the sand-bath with strong nitric acid to dissolve out excess of iron.

Analysis IX. On silver salt made from the crystallized sodium salt (No. 2) containing 19·91 grs. carbon.

Analysis X. Silver salt prepared from sodium salt (No. 4) containing 21·59 carbon, or in which the sodium was to the carbon as 1 : $5\frac{1}{2}$. In order if possible to remove the excess of iron, the salt was first precipitated by sulphate of copper and washed, the copper salt was now decomposed by soda and crystallized, and the silver salt was precipitated from this newly-crystallized portion.

I. 19·605 grs. gave 3·77 grs. $Fe^2 O^3$ and 12·86 grs. Ag Cl.
II. 16·795 grs. gave 3·24 $Fe^2 O^3$ and 10·94 Ag Cl.
III. 13·580 grs. gave 2·60 $Fe^2 O^3$ and 8·79 Ag Cl.
IV. 6·765 grs. gave 1·35 $Fe^2 O^3$ and 4·355 Ag Cl.

V. 14·68 grs. gave 2·80 Fe^2O^3 and 9·44 grs. Ag Cl.
VI. 13·16 grs. gave 2·43 Fe^2O^3 and 8·535 Ag Cl.
VII. 24·41 grs. gave 4·54 Fe^2O^3 and 15·79 Ag Cl.
VIII. 15·21 grs. gave 2·88 Fe^2O^3 and 9·89 Ag Cl.
IX. 13·60 grs. gave 2·60 Fe^2O^3 and 8·80 Ag Cl.
X. 8·81 grs. gave 1·69 Fe^2O^3 and 5·59 Ag Cl.

The combustions were made partly with chromate of lead, partly with oxide of copper.

I. 12·05 grs. gave 6·08 grs. CO^2 and 0·10 gr. HO.
II. 12·195 grs. gave 6·10 CO^2 and 0·08 HO.
IV. 8·10 grs. gave 4·03 CO^2 and 0·09 HO.
V. 10·35 grs. gave 5·13 CO^2 and 0·21 HO.
VI. 14·52 grs. gave 7·18 CO^2 and 0·05 HO.
VIII. 9·56 grs. gave 4·85 CO^2 and 0·04 HO.
IX. 10·835 grs. gave 5·50 CO^2 and 0·10 HO.

	I.	II.	III.	IV.	V.
Iron	13·46	13·50	13·40	13·97	13·35
Silver	49·42	49·02	48·71	48·46	49·50
Carbon	13·75	13·64	...	13·56	13·43
Hydrogen	0·09	0·07	...	0·12	0·22
Nitrogen, Oxygen	23·28	23·77	...	23·89	23·50
	100·00	100·00		100·00	100·00

	VI.	VII.	VIII.	IX.	X.	Mean.
Iron	12·92	13·01	13·25	13·38	13·42	13·36
Silver	48·67	48·69	48·93	48·70	47·77	48·78
Carbon	13·48	...	13·82	13·84	...	13·64
Hydrogen	0·03	...	0·04	0·10	...	0·09
Nitrogen, Oxygen	24·90	...	23·96	23·98	...	24·13
	100·00		100·00	100·00		100·00

If we assume the mean iron, 13·36, to represent the true quantity, then the silver to correspond to it in atomic proportion should have been 51·53, whereas there is only 48·78. Hence there is 0·72 of iron in excess over the equivalent quantity; this excess corresponds to $\frac{1}{18}$th of an equivalent. Again, supposing the carbon to be in the same proportion to the silver as in the nitroprussides, there should have been 13·0, so that there is an excess of 0·64. The excess of iron and of carbon is therefore almost exactly as 1 equiv. : 4 equivs., or viewing the carbon as representing cyanogen as 1 : 2. On this view the amount of impurity in the silver salt is 2·10 per

cent. Calculating the mean analysis deprived of this supposed impurity, we have

		Theory of nitroprusside of silver.
Iron	12·92	13·01
Silver	49·81	50·18
Carbon . . .	13·28	13·38
Hydrogen . .	0·097	0·18
Nitrogen, Oxygen .	23·02	23·25
	100·00	100·00

In the previous calculation the cyanide supposed to be present is $Fe\,Cy^2$; this only denotes the proportion of iron to the cyanogen; it is possible though less probable that it might be $2(Fe\,Cy + HCy)$. In this case we might suppose the analysed silver salts to contain this cyanide somewhat in the following proportion: 7 equivs. nitroprusside to 1 equiv. of the supposed cyanide. On this supposition the calculated and actual numbers would be as follows:—

	Calculated.	Mean.
Iron	13·50	13·36
Silver	49·26	48·78
Carbon . . .	13·76	13·64
Hydrogen . .	0·20	0·09

It is not however to be supposed that this cyanide is present as a chemical compound in the above proportion, as the differences in the analyses show that it occurs in varying and not very definite proportions.

It would indeed appear that the barium and sodium nitroprusside contained a body in which the iron and cyanogen are in the same proportion as in ferrocyanogen ($Fe\,Cy^3$). But as the silver salt precipitated from them does not contain an excess of carbon, it can scarcely be supposed that this would not be precipitated. But in fact there are no data further than the mere ultimate analyses upon which reasoning can be founded with regard to this dissolved and combined foreign substance in the partially decomposed nitroprussides. As however all their essential characters and their crystalline form remain altogether unaltered, we cannot view the foreign substances as more than accidental.

[To be continued.]

XXXIV. *On the Velocity of the Electrical Wave or Current through a Metallic Circuit. By* O. M. Mitchel, *Director of the Cincinnati Observatory**.

THE machinery now in use in the Cincinnati Observatory, for the conversion of time into space, furnishes the means of executing the most delicate experiments in the record of minute fractions of time. The sidereal clock is made to record its beats on a metallic disc, revolving beneath a steel recording pen fixed in position. The disc which carries the metal plate is made to revolve with uniform velocity, and receives the stroke of the recording pen without affecting its motion. A second pen, situated directly opposite the first, is placed under the control of the observer at the transit or other instrument, and gives him the means of recording any observed phænomenon with all the accuracy with which the eye can seize the instant of its occurrence.

On the completion of this machinery, several months since, my attention was called to the velocity of electrical currents in their passage along the telegraphic wires and through the ground, as being involved in the determination of differences of longitude by signals, transmitted telegraphically.

On the evening of the 12th of November, a series of experiments was performed at the Observatory, to determine the velocity of the electrical wave in its passage along the telegraphic wires. The long circuit involved in these experiments was formed as follows:—

From the main battery in the O'Rielly Telegraph Office, Cincinnati, along one wire to the observatory, a distance of one mile; thence, by the continuation of the same wire, to Pittsburg; thence, returning on a second wire, to the observatory; thence, through the receiving magnet, to a ground wire; thence, one mile, through the ground, to the main battery in Cincinnati.

The following is the plan on which the experiments were conducted. The sidereal clock was so arranged that its pendulum closed a local circuit, operating on the *time-pen* and recording the alternate clock-beats or seconds, on a metal plate placed on the revolving disc already described. This connexion remained unchanged during the entire course of the experiments, and this pen is called hereafter the *standard pen.*

A receiving magnet was made to close a short local circuit (equal in power and length to the former), which operated on the *observation pen*, causing it to strike its point into the metal

* From the American Astronomical Journal, No. 2, December 13, 1849.

plate. This receiving magnet was operated on in two modes, at the pleasure of the experimenter, as follows:—

1. By a local circuit, which was closed by the metallic handle of the standard pen.

2. By the long circuit before described, passing to and from Pittsburg, a distance of 607 miles, along the wires.

By these connexions, it will be seen that the clock-beats were directly recorded by the standard pen. They were also recorded by the *variable pen* (as I shall designate the second one), moved by the standard pen, closing either a short local circuit through the receiving magnet, or the long Pittsburg circuit, through the same receiving magnet,—this receiving magnet, as before stated, closing the local circuit operating on the variable pen.

The standard pen record was followed by the variable pen record, at an interval in time equal to the armature time of the standard pen, increased by the armature time of the receiving magnet, increased by the wave time of the fluid in passing through the short circuit and receiving magnet, this last being of course insensible. This statement applies when the variable pen is driven by the short local circuit.

When the long circuit operates on the receiving magnet, and through this on the variable pen, then the standard pen is followed by the variable pen at an interval identical with the preceding, increased by the time required by the electrical wave or current for traversing the wires 607 miles.

This statement is only true on the following conditions, viz.—

1. The intensity of the local circuit and the long circuit must be reduced to equality.

2. The adjustment of the receiving magnet must be constant, and its pass must be reduced to a minimum.

These two conditions being fulfilled, in case the two pens are so adjusted to each other in position that a straight line joining any two corresponding dots struck by them on a disc at rest will pass through the centre of the disc, then the interval between the records of the two pens driven by a short and long circuit, diminished by the interval between the records when the variable pen is driven by a short circuit, will exhibit the time occupied by the wave in traversing the distance of 607 miles through the wires.

I will now proceed to show the importance of fulfilling strictly these three conditions:—

1. To adjust to equality the intensity of the long and short circuits operating through the receiving magnet upon the variable pen.

2. To reduce the pass of the receiving magnet to a minimum, and to keep it unchanged.

3. To adjust the recording pens to exact radiation of records, the disc being quiescent.

To ascertain the effect of intensity and pass, the following experiments were performed. The connexions having been made as above described, four circumferences of second-dots were recorded by the two pens, under the following circumstances:—

No. 1. The pass of the receiving magnet a minimum.

No. 2. The pass of the receiving magnet a maximum.

No. 3. The battery reduced to one-half its former power, the pass a maximum.

No. 4. The reduced battery, the pass a minimum.

On circumference No. 1,—minimum pass, and strong battery,—the variable pen fell behind the standard pen $0^{s}{\cdot}091$ on a mean of many measures. The uniformity of these records and the accuracy of the measures are best exhibited by the measures themselves. I give as a specimen the first ten measures out of thirty:—

s	s		s
0·091	0·091		0·092
0·092	0·091		0·091
0·090	0·090	Mean	0·091
0·090	0·092		

On circumference No. 2,—maximum working pass, strong battery,—the same interval became $0^{s}{\cdot}2628$.

Circumference No. 3,—battery reduced one-half, maximum pass,—the same interval measured $0^{s}{\cdot}310$.

Circumference No. 4,—weak battery, pass a minimum,—same interval measured $0^{s}{\cdot}104$.

From these experiments, it becomes manifest that the adjustment of the receiving magnet may give variations of record far greater than the anticipated value of wave time on the longest available circuit. It is further shown, that the effect of different intensities is such as to entail an error so great as to render all experiments useless, from which this effect is not strictly eliminated.

Two difficulties were now to be overcome. The batteries must be reduced to equality, and the evidence of that equality must be obtained. The following plan was adopted to accomplish these objects. The handles of the recording pens are flexible, and vibrate at every stroke of the pen. Half the length of this vibration is the armature time, as I will show hereafter. Now the armature time was found to depend on the intensity of the currents which operated on the receiving

magnet. The local battery was therefore increased or decreased in power until the armature time, as recorded by the two circuits, was identical in value;—the pen being so adjusted that the primary dot was always followed by the second, or vibrating dot, which was distinctly recorded by the pen in every record made.

These difficulties being thus overcome, the pens were adjusted to produce radiating dots, the disc being quiescent. It was subsequently found that this adjustment was imperfect to eleven thousandths of a second of time on the greatest circumference recorded. The absolute space was laid off on all the remaining circumferences, and, being accurately measured, gives the required correction.

All the arrangements having been perfected, the local connexions were placed in the care of Mr. Henry Twitchell, while it was assisted in the distant connexions by Mr. Stager, of the O'Rielly Telegraph Office, Cincinnati.

The evening was fair and calm, warm for the season. Mr. Stager reported the line in admirable working order. The receiving magnet was adjusted to its minimum working pass, and the long and short circuit batteries were pronounced equal in strength, by the equality of the recorded armature times.

At $9^h\ 58^m$ the pens fell together on the metal plate, the variable pen being operated on by the long circuit. I watched the disc to see that the records were perfectly made. The dots came down in the most beautiful manner, and the record was engraved on the metal with exquisite delicacy. At the close of the first circumference of dots, occupying exactly sixty seconds, notice was given to change; and, at the word, the long circuit was thrown off, and the local short circuit took its place. This change was so skilfully accomplished by Mr. Stager, that not a second was lost.

In this way five complete circumferences were recorded,—three with the long, and two with the short circuit. The ear could sometimes with difficulty recognize the change from long to short; but after many trials it was found that this organ could not with certainty be relied on. The conversion of time into space on the disc gave us, however, the opportunity of bringing a high magnifying power to bear upon the reading of the delicate records.

Mr. Twitchell has completed all the measures with the instrument contrived by me for measuring small angular spaces. It can be read down to the thousandth of a second of time. The disc performed in the most admirable manner during the entire experiment, the records radiating from its centre, and demonstrating the uniformity of its motion.

I present the measures of circumferences Nos. 1 and 2, to

exhibit the character of the records. The slight variations are due, doubtless, to want of absolute uniformity in the connexions. The variations are, however, very slight, and disappear in the mean of a group of thirty observations. The measures follow.

Circ. No. 1. Measured interval from standard pen to variable pen.	Circ. No. 2. Measured interval from standard pen to variable pen.	Differences No. 1—No. 2.	Correction for non-radiation—constant. No. 1.	No. 2.
s	s	s	s	s
0·055	0·040	0·015	0·0110	0·0145
0·050	0 039	0·011		
0·055	0·036	0·019		
0·054	0·040	0·014		
0·054	0·040	0·014		
0·056	0·039	0·017		
0·055	0·039	0·016		
0·054	0·040	0·014		
0·055	0·040	0·015		
0·055	0·041	0·014		
0·055	0·040	0·005		
0·056	0·038	0·018		
0·056	0·040	0·016		
0·057	0·038	0·019		
0·061	0·046	0·015		
0·060	0·041	0·019		
0·061	0·040	0·021		
0·060	0·040	0·020		
0·058	0·041	0·017		
0·058	0·040	0·018		
0·060	0·041	0·019		
0·059	0·041	0·018		
0·058	0·039	0·019		
0·057	0·040	0·017		
0·057	0·039	0·018		
0·058	0·039	0·019		
0·057	0·040	0·017		
0·060	0·040	0·020		
0·057	0·040	0·017		
0·056	0·040	0·016		
Mean 0·0568	0·0399	0·01656		
0·0110	0·0145			
0·0458	0·0254			

The mean values of the intervals are as follows:—

	Correction for non-radiation.
	s s s
No. 1, long circuit, interval	$=0·0568-0·0110=0·0458$
No. 2, short ...	$=0·0399-0·0145=0·0254$
No. 3, long ...	$=0·0633-0·0165=0·0468$
No. 4, short ...	$=0·0444-0·0195=0·0249$
No. 5, long ...	$=0·0682-0·0215=0·0467$

From a comparison of these values we obtain—

	s
No. 1, long circuit	=0·0458
No. 2, short circuit	=0·0254
Wave time on 607 miles of wire	=0·0204
No. 1	=0·0458
No. 4	=0·0249
Wave time	=0·0209
No. 3	=0·0468
No. 2	=0·0254
Wave time	=0·0214
No. 3	=0·0468
No. 4	=0·0249
Wave time	=0·0219
No. 5	=0·0467
No. 2	=0·0254
Wave time	=0·0213
No. 5	=0·0467
No. 4	=0·0249
Wave time	=0·0218

Wave time on 607 miles of wire, as deduced from—

	s
No. 1 – No. 2	=0·0204
No. 1 – No. 4	=0·0209
No. 3 – No. 2	=0·0214
No. 3 – No. 4	=0·0219
No. 5 – No. 2	=0·0213
No. 5 – No. 4	=0·0218
Mean	=0·02128

	s
Mean ± No. 1	= 0·00088
Mean ± No. 2	= 0·00038
Mean ± No. 3	= 0·00014
Mean ± No. 4	= 0·00064
Mean ± No. 5	= 0·00002
Mean ± No. 6	= 0·00052
Mean	= 0·00043

That is, the mean difference from the mean amounts to 43 hundred-thousandths of a second of time.

The velocity deduced along the wires, in case the circuit is 607 miles in length, is 28524 miles per second.

At some future time I hope to resume these investigations. It would be interesting to repeat these experiments at differ-

ent hours of the day and night, at different seasons of the year, and through different media. May not the velocity through the ground vary with the direction of the current,—whether east and west, or north and south?

I place great confidence in these results, as every care was taken to eliminate all possible sources of error. Every magnet in use was in the observatory, and all the connexions and adjustments were under my own eye.

The adjustment of the receiving magnet was unaltered during the experiment, and no change occurred for more than thirty minutes after the experiments were finished. Each of the pens recorded, in every instance, the armature time in the distance from the primary to the vibratory dot. One-half this record was shown to be the armature time, as follows:—

The variable pen could not begin to descend until the standard pen was down. Hence, considering the armature time of the receiving magnet as insensible (as it was), with a short circuit the interval of the two records would be equal to the armature time of the standard pen. This interval, being measured, was found to be exactly one-half the interval between the primary and vibratory dot of the standard pen.

The length of this article forbids me to go into further detail. In case further particulars are desired, it will give me pleasure to furnish them by correspondence, or by a further publication.

Cincinnati Observatory,
November 16, 1849.

XXXV. *On the True Amplitude of a Tessarine; on the Derivation of the word* Theodolite; *and on Light under the action of Magnetism. By* James Cockle, *Esq., M.A., of Trinity College, Cambridge, Barrister-at-law**.

ANY tessarine $w+i'x+j'y+k'z$, or t, may be put under the form†

$$M\{q\cos p+i'r\sin p+j'(1-q)\cos p+k'(1-r)\sin p\},$$

* Communicated by the Author. Mr. Cockle takes an opportunity of stating that remarks on the Tessarine Theory, as well as on the impossible equations and quantities so intimately connected with it, will be found in his *Horæ Algebraicæ*, published in vols. xlvii. to l. of the Mechanics' Magazine; and he also begs to refer the reader to the following papers published in the same work, and in which he has adverted to the same subject; viz. "On Algebraic Symbols," Mechanics' Magazine, vol. l. pp. 292–294; "On the Symbols of Algebra, and on the Theory of Tesssarines," Ibid. p. 534; "On the Tessarine Algebra," Ibid. pp. 558, 559; "On certain Researches of Mr. Boole, and the Symbol of Infinity," Ibid. vol. li. pp. 124, 125; "On Systems of Quadruple Algebra," Ibid. pp. 197–199; and see some further remarks "On Quadruple Algebra," Ibid. vol. li. pp. 557, 558; and "On Tessarines," Ibid. p. 610.

† See Mechanics' Magazine, vol. li. p. 610.

provided that in the expression for the true modulus M (which may be seen at p. 435 of the preceding volume of this Journal) both the undetermined signs are taken as positive. Let M, p, q, r be called the ELEMENTS of the tessarine t; then, if M$'$, p', q', r' be the elements of t', and M$''$, p'', q'', r'' those of t''; and, further, if t, t', and t'' be connected by the relation $tt'=t''$, the following equations will hold between the elements of the product and of the factors; viz.

$$q''\cos p''=\{1-(q,q')\}\cos p\cos p'-\{1-(r,r')\}\sin p\sin p',$$

$$r''\sin p''=\{1-(q,r')\}\cos p\sin p'+\{1-(q'r)\}\cos p'\sin p,$$

$$(1-q'')\cos p''=(q,q')\cos p\cos p'-(r,r')\sin p\sin p',$$

$$(1-r'')\sin p''=(q,r')\cos p\sin p'+(q',r)\cos p'\sin p,$$

where (a, b) denotes $a+b-2ab$. The above four equations will readily be verified by a comparison with those which I gave (at p. 437 of vol. xxxiii. of the Phil. Mag. S. 3) as connecting the *constituents* of the product with those of the factors; add the first and the third of them together, and we have

$$\cos p''=\cos p\cos p'-\sin p\sin p'=\cos(p+p'), \quad . \quad \text{(a.)}$$

whence, calling p the *true amplitude* of t, we infer that the true amplitude of the product of two tessarines is the sum of the true amplitudes of the factors. And we might have inferred the same thing by adding the second and fourth of the above equations, which would have given us

$$\sin p''=\sin(p+p'). \quad . \quad . \quad . \quad . \quad . \quad \text{(b.)}$$

The combination of (a.) and (b.) shows us, however, that, rejecting circumferences, $p''=p+p'$ is the only relation between p, p', and p''. It will be remembered, in the above investigation, that M$''$=MM$'$.

I may add that these relations, both between the moduli and the amplitudes of systems of tessarines, may be readily arrived at by combining the reasoning of paragraphs 10 to 12 of the Rev. Professor Charles Graves's paper on Triple Algebra at pp. 119–126 of vol. xxxiv. of this Journal*, with that used by

* In the same Number (Phil. Mag. for February 1849) I have given (see pp. 132–135) a "Solution of Two Geometrical Problems," by means of a Uniaxal Geometry. The rationale of the *virtual solution* is clear and logical. Let it be required to find a point situate at a distance a from a given point (the origin, for instance), and fulfilling certain (specified) conditions. In Uniaxal Geometry the solution proceeds by supposing the required point to be situate in the primary axis or *axe*—as to which latter term see Phil. Mag. S. 3. vol. xxxiv. pp. 408, 409. And on this supposition we find for the distance of the required from the given point an expression of the form

$$A+B\sqrt{-1} \text{ (or, more generally, } A+B\sqrt{-1}+Cj);$$

him in the opening of his "Abstract of a Paper on Algebraic Triplets," &c. in part 1 of vol. iii. of the Proceedings of the Royal Irish Academy, where he establishes similar relations between the moduli and amplitudes of couplets.

On the Derivation of the word Theodolite.

Professor De Morgan has (see Phil. Mag. S. 3. vol. xxviii. pp. 287–289) upon this subject offered some remarks which attracted my attention to the point, and induced me to make a conjecture as to the derivation, which I communicated to the Mechanics' Magazine*. Mr. De Morgan's remarks have given an interest to the topic; and to this Journal, as the most fitting place, I have ventured to transmit these comments upon his remarks.

The question whether the word *theodolite* is derived from *alidade* entails upon us (not necessarily perhaps, but sufficiently) the consideration of the point (1) whether the instrument called *alidade* or *alhidada* is *peculiar* to the theodolite; and this latter point again conducts to the further question (2), whether the alhidada differs from what is termed in the *Pantometria* the "index with sightes."

It will be convenient to consider the latter question first. Let us turn to "The. 22. Chapter." of the "fyrst Booke" (*Longimetra*) of the *Pantometria* (edition of 1571). This chapter treats of "The making of an Instrument named the Geometricall square." And, among other instructions, we are told "..., forget not to haue an index, not with commune sightes, but thus, .." Then follow directions for their construction, and we are informed respecting the index that "it hath place in the cêtre, and there made to tarry, so that with ease it may be turned from the first to any pointe." This is the "index with sightes" as it is afterwards expressly called in chapter 29 of the *Longimetra*. And, if there be any difference between this *index with sightes* and an *alidade*, it must consist in this,—that the index has *one extremity fixed to the centre* of the circle of which a quadrant is employed, while the alidade, which may be regarded as a *double index*, has its centre fixed to the centre of the circle to which it appertains. In effect, if not precisely in form, the index must be a revolving *radius*, and the alidade a revolving *diameter*; each furnished with "sightes." Mr. De Morgan, in his paper above

and if $A^2+B^2=a^2$ (or a^2-C^2, as the case may be), we see that the point so determined will be situate at a distance a from the given one, though *not in the* AXE. But, this last condition not being essential to the inquiry, the virtual solution is complete. This is the theory of the Virtual Solution.

* See pp. 159, 160 of vol. xlv. (No. 1201) of that work.

referred to, applies the term alidade to a revolving diameter. At least, such seems to be the effect of his statement, that "A ruler with sights, travelling upon a graduated circle, was a constituent part of various astronomical instruments imported into Europe from the East, and was accompanied by the Arabic term *alhidada* to express it:" combined with a remark in the paragraph preceding that statement. But it will be observed that it is only by implication (if, indeed, at all) that Mr. De Morgan excludes the application of the term *alidade* to a revolving *radius*, and, consequently, without contradicting his high authority, I may express a doubt whether any more is essential to the definition of an alidade than that it is a ruler with sights travelling on a circle *or portion of a circle*, and that it is as applicable to a revolving radius as to a revolving diameter. It is true that, in chapter 29 of *Longimetra*, the term "index with sightes" is used with reference to the "square Geometricall," and "Alhidada" with reference to "Theodelitus;" but then the words "er* index with sightes" immediately follow the word "alhidada," and would rather seem to show that the expressions are synonymous. Why, then, are they apparently distinguished in the manner just mentioned? It may be said, because the alhidada is a double index, whose diversity from the single one is manifested by the first plate to chap. 29, where the alidade occupies the right, and the index the left-hand side. But on the other side it may be urged, and perhaps not without effect, that, although the word "Theodelitus" occurs before that chapter, yet the term "alhidada" does *not*; and further, that the more complicated nature of the "instrument Topographicall" described in chap. 29 renders the use of the word *alhidada* necessary, in order to distinguish the index of the theodolite from that of the square, and that it is only for convenience and distinctness that it is there used.

The view contained in the preceding paragraph appears to be confirmed by a consideration arising upon the first of the points above alluded to. It will be found that the chapter 27 of *Longimetra* ("The composition of the instrument called Theodelitus") contains no mention whatever of the term alidade. We are told that "The index of that instrument with the sightes, &c. are not unlike to that whiche the square hath: . ." No *peculiarity* of its index is adverted to either in that or in the succeeding (28th) chapter, in which the "index" is mentioned no less than three times. Add to this, that the *theodelitus* may (*Long*. chap. 27 and 28) be *semicircular*, in which case a *single index* would be used, and the alidade (even if it meant a *double* index) would cease to be identified

* *Sic* in original. Probably a misprint for *or*?

with the instrument; and that other instruments into whose construction circles entered—such as Astrolabes—would also be furnished with *alidades*, and we may perhaps be justified in entertaining some doubt as to the connexion of the words theodolite and alidade.

It will be noticed that Bourne's "Treasure for Travailers," cited by Mr. De Morgan, was published in 1578, seven years after the first edition of the *Pantometria*, in which, so far as I am aware, the term *athelida* does not occur. Might not the term *theodelitus*, which was prior in point of publication, suggest *athelida*?

Adopting, as I do, Professor De Morgan's view, that *theodelitus* is an adjective or a participle, I think that fact favourable to my own derivation. The *graduation* of the instrument appears to be its principal feature in the eye of the writer of the *Pantometria*. "It is," he says, "but a circle divided," . . "or a semicircle parted.." Now if we pass from ὀβελὸς, ὀβελίζω to the Doric or Æolic forms ὀδελὸς, ὀδελίζω, we have in the word *odelited* the very expression of a *graduated* instrument. The prefix *The* may either be a redundancy, or connected with θεάομαι, in which latter case the instrument would be described as a *graduated seer*, or, what would be equivalent, a *seer of graduations or angles*. The graduation may, as a criterion, be liable to as much ambiguity as the being furnished with an alidade, but it provides us with a singular approximation in sound and meaning.

On Light under the action of Magnetism.

At pp. 469–477 of vol. xxviii. of the present series of this Journal, the Astronomer Royal has given equations applying to light under the action of magnetism. Some time since I made a communication, on the subject of Mr. Airy's investigations, to a cotemporary Journal*, but the following observations may not be misplaced here.

Of the species of force to which that alluded to by Mr. Airy in the last paragraph but one of his letter (Phil. Mag. S. 3. vol. xxviii. p. 477) belongs, we have a striking instance in *centrifugal force*. Conceive a particle attached to a fixed point by an inextensible string, and revolving round the point. The tension of the string depends upon the velocity of the particle in the direction of its motion—which is always *perpendicular* to the string. This induced me to enter into some investigations on the subject, which I discontinued; for I found that some researches of Mr. O'Brien (in this Journal, vol. xxv. pp. 326–334) might be adapted to the same end. If (Ib. p. 331) we

* See Mechanics' Magazine, vol. xlvii. pp. 575, 576.

make $T=0$, and suppose the polarization circular, we readily arrive, after some necessary substitutions, at the Astronomer Royal's results. I hope that I am not too rash in adding, that it would seem that those results exclude the idea of there being anything like *friction* between the particles of the luminiferous æther and those of the glass. At least they would do so if the equations held for any considerable portion of the path of the ray—if they did not hold they might, it seems to me, be made to furnish a measure of the friction, and so aid us in forming a mechanical theory.

2 Pump Court, Temple,
February 16, 1850.

XXXVI. *On a new Equation in Hydrodynamics, in Reply to* Professor P. Tardy. *By the* Rev. J. Challis, *M.A., F.R.S., F.R.A.S., Plumian Professor of Astronomy in the University of Cambridge**.

THE observations communicated by Professor P. Tardy of Messina to the March Number of the Philosophical Magazine, on a new equation which I have asserted to be necessary to complete the analytical principles of hydrodynamics, proceed evidently from a mathematician who is well able to discuss this difficult question, and have received from me the most careful consideration. I shall endeavour to reply to them as nearly as possible in the order in which they occur in Professor Tardy's communication.

In the first place, I fully admit that Professor Tardy has proved that the equation

$$\frac{d\rho}{dt}+\frac{d.\rho V}{ds}+\rho V\left(\frac{1}{r}+\frac{1}{r^1}\right)=0 \quad . \quad . \quad . \quad . \quad (1.)$$

may be obtained without making use of my new equation, viz.

$$\frac{d\psi}{dt}+\lambda\left(\frac{d\psi^2}{dx^2}+\frac{d\psi^2}{dy^2}+\frac{d\psi^2}{dz^2}\right)=0. \quad . \quad . \quad . \quad (2.)$$

I arrived at (1.) after first eliminating λ by means of (2.) from the equations

$$u=\lambda\frac{d\psi}{dx},\quad v=\lambda\frac{d\psi}{dy},\quad w=\lambda\frac{d\psi}{dz};$$

but I did not remark, what indeed the process sufficiently indicated, that the result was independent of the value of λ employed, and I concluded erroneously that (2.) was necessary

* Communicated by the Author.

for obtaining (1.). It must, however, be observed, that Professor Tardy assumes the existence of the function λ, without which it would not be possible to arrive at the equation (1.), which is admitted to be a true equation, and yet he has not shown in what way that function may be found. There can be no doubt that it is a discoverable quantity; and since the number of recognized equations is only equal to the number of the other unknown quantities, it follows that another equation is necessary to make up the requisite number of equations. Consequently the necessity for a new equation results from Professor Tardy's reasoning.

Respecting equation (1.) I have further to remark, that it is not, as Professor Tardy states it to be, merely an analytical transformation of the equation

$$\frac{d\rho}{dt}+\frac{d.\rho u}{dx}+\frac{d.\rho v}{dy}+\frac{d.\rho w}{dz}=0, \quad . \quad . \quad . \quad . \quad . \quad (3.)$$

for in that case such a function as λ would not be required for deducing it. It differs from (3.) by containing an explicit expression of the principle, that the directions of motion in any given element at any time are normals to a continuous surface, which principle is tacitly adopted when we assume that the differential calculus is applicable to the consideration of fluid motion. The equation (1.) cannot be obtained without expressly taking account of *surfaces of displacement*, the consideration of which has usually been omitted by writers on hydrodynamics.

I admit also that the reasoning which Professor Tardy is unable to appreciate, viz. that by which I derive the equation (2.) from fundamental principles, requires further elucidation; and I will now endeavour to place it in a clearer light. Let it be granted that the directions of motion in any given element of the fluid at any time are normals to a continuous surface. Then, ψ being an unknown function of the co-ordinates and the time, it follows from this principle that u, v and w are respectively in the proportion of $\frac{d\psi}{dx}$, $\frac{d\psi}{dy}$, and $\frac{d\psi}{dz}$; or, λ being another unknown function of the co-ordinates and the time, that

$$u=\lambda\frac{d\psi}{dx}, \quad v=\lambda\frac{d\psi}{dy}, \quad w=\lambda\frac{d\psi}{dz}.$$

Hence

$$(d\psi)=\frac{u}{\lambda}dx+\frac{v}{\lambda}dy+\frac{w}{\lambda}dz.$$

Thus the right-hand side of this equality is integrable in con-

sequence of the adoption of the principle just enunciated, so that there is no occasion for the application of the criterion of integrability by a factor. Now, as is well known, one fundamental hydrodynamical equation rests on the principle that the mass of a given element in motion *continues* from one instant to the next to be the same. That is, if $\rho\, dx\, dy\, dz$ be the mass of the element, this equation of continuity is

$$\delta . \rho dxdydz = 0,$$

the symbols of operation δ and d being independent of each other, and the former having reference to time as well as space. This equation is equivalent to (3.). In an analogous manner, another fundamental equation rests on the principle that a certain geometrical condition, viz. that the directions of motion in a given element are normals to a continuous surface, *continues* from one instant to the next. That condition is analytically expressed by the equation

$$(d\psi) = 0,$$

and the second equation of continuity consequently is

$$\delta . (d\psi) = 0, \qquad \ldots \ldots \quad (4.)$$

the symbols δ and d being used in the manner stated above. Now

$$\delta . (d\psi) = \delta . \frac{d\psi}{dx} dx + \delta . \frac{d\psi}{dy} dy + \delta . \frac{d\psi}{dz} dz.$$

And

$$\delta . \frac{d\psi}{dx} dx = \left(\frac{d^2\psi}{dxdt} \delta t + \frac{d^2\psi}{dx^2} \delta x + \frac{d^2\psi}{dxdy} \delta y + \frac{d^2\psi}{dxdz} dz \right) dx + \frac{d\psi}{dx} d\delta x.$$

By adding to this the analogous expressions for $\delta . \frac{d\psi}{dy} dy$ and $\delta . \frac{d\psi}{dz} dz$, the result by reason of equation (4.) is

$$0 = \left(d . \frac{d\psi}{dt} \right) \delta t + \frac{d\psi}{dx} d\delta x + \left(d . \frac{d\psi}{dx} \right) \delta x$$
$$+ \frac{d\psi}{dy} d\delta y + \left(d . \frac{d\psi}{dy} \right) \delta y$$
$$+ \frac{d\psi}{dz} d\delta z + \left(d . \frac{d\psi}{dz} \right) \delta z,$$

or

$$0 = d . \left(\frac{d\psi}{dt} \delta t + \frac{d\psi}{dx} \delta x + \frac{d\psi}{dy} \delta y + \frac{d\psi}{dz} \delta z \right).$$

Since by hypothesis the reasoning applies to a *given* element,

$$\delta x = u \delta t, \quad \delta y = v \delta t, \quad \delta z = w \delta t.$$

Hence substituting in the above equation and integrating, we obtain

$$0=\frac{d\psi}{dt}+\frac{d\psi}{dx}u+\frac{d\psi}{dy}v+\frac{d\psi}{dz}w+\chi(t). \quad . \quad . \quad (5.)$$

Lastly, by taking account of the equalities,

$$u=\lambda\frac{d\psi}{dx}, \quad v=\lambda\frac{d\psi}{dy}, \quad w=\lambda\frac{d\psi}{dz},$$

and supposing the arbitrary function of the time to be included in ψ, we have the equation (2.) which it was required to deduce.

It may here be remarked, that the equation (5.) may be put under the form

$$\left(\frac{d\psi}{dt}\right)+\chi(t)=0.$$

Whence, by integration,

$$\psi+\chi_1(t)+C=0,$$

an equation which applies to a *given* element. Hence since C is a quantity altogether arbitrary, it cannot be affirmed that ψ has necessarily the same value at the same time for different elements. Again, if δx, δy, δz be the variations of the co-ordinates at a given time from the point xyz to any point indefinitely near, we have

$$(\delta\psi)=\frac{d\psi}{dx}\delta x+\frac{d\psi}{dy}\delta y+\frac{d\psi}{dz}\delta z.$$

Hence integrating along an arbitrary line,

$$\psi=\psi(x, y, z, t)+\Psi-\psi(a, b, c, t),$$

Ψ being the value of ψ at the arbitrary point abc at the given time. This equation gives the value of ψ at the point xyz; but as this value contains the arbitrary quantity $\Psi-\psi(a,b,c,t)$, it cannot be affirmed that ψ has necessarily the same value at the same time for any other point of space. From these two results we may conclude that the value of ψ is in no respect limited by the investigation which has conducted to the equation (2.).

I am quite at a loss to understand how any statement made by me in the Supplementary Number of the Philosophical Magazine for last June, should have led Professor Tardy to suppose that, according to my views, "the particles of the fluid which are on the surface $\psi=0$ remain on it during successive instants." I distinctly affirmed, what the preceding investigation has perhaps made more apparent, that the new equation "expresses the condition that the directions of the

motion in a *given* element are in successive instants normals to *surfaces* of continued curvature." The condition stated in other words is, that a continuous surface of displacement may be drawn through a given element in each of its successive positions. This law is just as conceivable in steady motion, in which the surfaces of displacement are fixed in space, as in instances of motion in which they are not fixed.

I come now to the argument by which Professor Tardy obtains a peculiar form of ψ by assuming that

$$\frac{R^2}{\frac{d\psi}{dt}} \text{ and } \frac{R^2}{\frac{d\psi}{dt}+\chi(t)}$$

are at the same time factors which render integrable $udx+vdy+wdz$, R^2 being substituted for

$$\frac{d\psi^2}{dx^2}+\frac{d\psi^2}{dy^2}+\frac{d\psi^2}{dz^2}.$$

It is true, as the investigation of equation (2.) has already shown, that the value of $\frac{1}{\lambda}$ generally expressed is

$$\frac{R^2}{\frac{d\psi}{dt}+\chi(t)}.$$

But, this being the value of $\frac{1}{\lambda}$, $\frac{R^2}{\frac{d\psi}{dt}}$ is *not* the value of $\frac{1}{\lambda}$; and as my investigation has regard only to the value of $\frac{1}{\lambda}$, it is not chargeable with the consequences of the particular assumption, that another quantity renders $udx+vdy+wdz$ integrable simultaneously with $\frac{1}{\lambda}$. Such assumption may be admissible and the consequences true, but they have no bearing upon the question at issue; besides which, I have anticipated this argument by proving already, that the investigation which conducted to equation (2.) in no respects limits the value of ψ.

Next follows an example which is thought to show evidently the inconsistency of the new equation. The result which I am called upon to justify is a legitimate deduction from my equation; and as Professor Tardy finds no fault with it, neither do I. The meaning of the result I take to be this. The two equations

$$x^{t-1}.y^{t+1}+\chi(t)=0,$$

$$\log xy+(t-1)^2\frac{y}{x}+(t+1)^2\frac{x}{y}-\frac{\chi'(t)}{\chi(t)}=0,$$

serve to determine x and y as functions of the arbitrary quantity $\chi(t)$. These values, substituted in the equations $u=y(t-1)$, $v=x(t+1)$, show that the velocities are functions of the same arbitrary quantity. Hence the proposed instance is one of *constrained* motion, possible under given circumstances.

The above interpretation will perhaps be made plainer by taking a simpler example. Let $u=mx$, $v=-my$, the fluid being incompressible and no force acting. Then it follows from the equations $\frac{u}{v}=\frac{dx}{dy}=-\frac{x}{y}$, that the lines of motion are rectangular hyperbolas, and consequently, by mere geometrical considerations, that the lines of displacement are also rectangular hyperbolas of which the general equation is $x^2-y^2-a^2=0$. Hence the equation $\psi=0$ becomes

$$\psi=x^2-y^2+\chi(t)=0.$$

Hence $$\frac{d\psi}{dt}=\chi'(t), \frac{d\psi}{dx}=2x, \frac{d\psi}{dy}=-2y, \lambda=\frac{u}{\frac{d\psi}{dx}}=\frac{m}{2}.$$

Consequently the new equation becomes for this instance,

$$\chi'(t)+2m(x^2+y^2)=0.$$

These two equations show that this is also an instance of constrained motion. I have remarked in the Cambridge Philosophical Transactions, vol. viii. part 1 (Art. 3 of my paper on a new Hydrodynamical Equation), that from the value of the pressure p, to which this example leads, viz.

$$p=f(t)-\frac{m}{2}(x^2+y^2),$$

it follows, by putting $p=0$, that the boundary of the fluid is at all times circular, a result incompatible with the hyperbolic lines of motion. This contradiction, I have asserted, and still assert, *proves* the insufficiency of the recognized hydrodynamical equations. The contradiction is removed by using the new equation. I think Professor Tardy would find it difficult to show how, in the instance he has selected, supposing $W=0$, the boundary can at all times be a rectangular hyperbola.

Further to illustrate this point, let us take another instance. Let

$$u=\frac{x}{r}\varphi(r), \quad v=\frac{y}{r}\varphi(r), \quad r^2=x^2+y^2.$$

Then plainly

$$\psi=x^2+y^2+\chi(t)=0,$$

$$\frac{d\psi}{dt}=\chi'(t),\ \frac{d\psi}{dx}=2x,\ \frac{d\psi}{dy}=2y,\quad \lambda=\frac{u}{\frac{d\psi}{dx}}=\frac{\varphi(r)}{2r}.$$

Hence the equation (2.) becomes for this instance

$$\chi'(t)+\frac{2\varphi(r)}{r}(x^2+y^2)=0;$$

or

$$\chi'(t)+2r\varphi(r)=0.$$

Hence

$$\varphi(r)=-\frac{\chi'(t)}{2r}.$$

In this case the two equations do not determine particular values of x and y, the motion not being entirely arbitrary, but such as may arise from the mutual action of the parts of the fluid on each other. While, according to the first equation, the lines of displacement are concentric circles, the second equation shows that the velocity at a given instant varies inversely as the distance from the centre.

The "illogical process" by which I have obtained the value of λ in the example in which $u=mx$ and $v=-my$, has been employed in several other instances in the same paper, and has always conducted to a correct result. I confess, however, that it contains the same fault as that which Professor Tardy has pointed out in the investigation of equation (1.), viz. that of needlessly introducing the equation (2.). When u, v, w are given explicitly, it is possible to obtain the general equation of the lines of motion, and by consequence the general equation of the surfaces of displacement, by geometrical reasoning. In such instances, therefore, ψ may be supposed to be given, and λ may be at once inferred from one of the expressions,

$$\frac{u}{\frac{d\psi}{dx}},\quad \frac{v}{\frac{d\psi}{dy}},\quad \frac{w}{\frac{d\psi}{dz}}.$$

This, in fact, is what I have virtually done, the equation (2.) not being really made use of.

Professor Tardy has justly criticized the assumption $ds=dr=dr'$. This assumption can be made only after proving that the trajectory of the surfaces of displacement is either momentarily or permanently a straight line. On the supposition that $udx+vdy+wdz=(d\varphi)$, it may be shown as follows, by means of the equation (2.), that the orthogonal trajectory is rectilinear. Since $udx+vdy+wdz=(d\varphi)=\lambda(d\psi)$, it is plain

that λ is some function of ψ and t. Hence equation (2.) becomes

$$\frac{d\psi}{dt} + \mathrm{F}(\psi, t)\frac{d\psi^2}{ds^2} = 0;$$

and consequently ψ is a function of s and t. The equation $\psi=0$ is therefore equivalent to $s=f(t)$, which equation cannot be true of all the lines of motion unless they are rectilinear. Hence when $udx+vdy+wdz$ is an exact differential, it is allowable to say that $ds=dr=dr'$; and thus the equations

$$\mathrm{V}=\frac{\varphi(t)}{rr'} \text{ and } \mathrm{V}=\frac{\varphi(t)}{r},$$

applying respectively to motion in three and in two dimensions of space, are legitimately inferred from the equation

$$\frac{d\mathrm{V}}{ds} + \mathrm{V}\left(\frac{1}{r} + \frac{1}{r'}\right) = 0.$$

As, however, these results have been obtained prior to the consideration of any particular instance of motion, they can only relate to the general law of the unconstrained action of the parts of the fluid on each other. In maintaining this result, I am far from asserting that instances of curvilinear motion may not take place under given arbitrary circumstances, when $udx+vdy+wdz$ is an exact differential.

I have now adverted to all the points of any moment contained in Professor Tardy's communication. While I admit that Professor Tardy has detected faults in my reasoning, I maintain that nothing has been urged which in the slightest degree invalidates the new equation; but, on the contrary, that the correction of these errors, pardonable perhaps in a new field of research, has only served more effectually to establish the truth of that equation.

Cambridge Observatory,
March 20, 1850.

[Subsequently to the publication of our last Number in which Professor Tardy's paper appeared, and therefore too late for insertion, we received from him the following Note, which we thought it right to communicate to Prof. Challis as soon as he informed us of his intention to reply.—Ed.]

"I have taken the equation $\mathrm{V}=\dfrac{\mathrm{A}}{rr'd\tau\, d\tau'}$ without examination from Professor Amici's Course of Hydraulics; but having had occasion to consider the matter more at leisure, I have found that the above expression is inadmissible."

XXXVII. *On the Electricity of Condensation.* *By* Reuben Phillips, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I FIND I have not been sufficiently explicit in giving my views of the forces concerned in the production of the electricity of condensation, and I perceive this from the question put by Mr. Birt, page 171 of the last Number of the Philosophical Magazine, namely, "Would the condensation or running together of the vapour particles cause an increase of tension?" Or as the question may be put, Is the electric tension really observed in experiments of the electricity of condensation only equal to, or less than, that of the electric tension of the molecules when first condensed?

It is shown (11.) that a jet of steam escaping into the air acts on a magnet like an electric current, and (26, 29.) sufficiently shows this effect to be occasioned by the condensation of steam, and for the reasons detailed (32.) these electric currents must be of a discontinuous molecular nature: or in other words, we have one set of particles positive and the other negative, with currents between them; unless, indeed, we suppose the resistance to the currents to be nothing, in which case the phænomenon would be entirely detached from what I have called the electricity of condensation. I have found that when drops of water are driven through this magnetic jet of steam (55, 56, &c.), the drops become powerfully electrified positively, at the same time the steam and water together may be very neutral, as in some experiments with the gun-barrel (58.), and could doubtless be made as nearly neutral as we please; therefore the particles of water being positive, another set of particles must have an equivalent negative charge. It is, I think, impossible for the drops of water to have become charged in any other way than by taking on themselves some of the positive electricity previously belonging to the particles concerned in producing the magnetic effect. Thus it is seen that the addition of drops of water to the condensing steam has only altered the general state of affairs by effecting a sort of separation between the positive and negative particles.

It now remains to point out that the electric tension derived from these aqueous drops is much higher than that of the positive molecules from which the charge was obtained.

The electric charge communicated to the electrometer by the steam in such experiments as those of (52, 56.) was so strong, that although I have not performed the experiment,

I have no doubt a spark can be obtained; that is, the electricity is powerful enough to pass from particle to particle through a visible interval of air. But the electricity so existed in the condensed vapour before the positive electricity passed to the drops of water, that the positive and negative particles were separated by an insensible space; from which it follows, that the electricity must then have been much weaker; and this increase of tension can, as it appears to me, only have taken place "in the way suggested by the Committee of Physics of the Royal Society."

But it may perhaps seem that this exalted tension is produced rather by the running together of the electrified drops of water than of the vapour particles; however, it must be observed, that the combination of two or more drops of the electrified water into one mass, so as to produce a diminished surface, is electrically equivalent to an approximation of the minute vapour particles which they contain, and on which we may imagine for this purpose the charge to exist.

It immediately appears from the foregoing, that the simple formation of cloud cannot generate any electrical force exterior to the cloud, as the positive and negative forces which it *may* contain (for the electro-current of condensation shows that after a time they may neutralize each other) must balance one another; and it can only be when the positive particles are removed from the negative by the carrying action of rain, that the static electric force is developed. It is, however, very possible that a cloud may collect and condense the general electricity of the atmosphere, or it may by specific induction cause the inductric action of the heavens to be concentrated under the cloud. It is, I think, from one or both of these causes that Mr. Birt's remark must be explained, "that generally before a shower an atmospherical conductor indicates the presence of negative electricity." (Phil. Mag., vol. xxxvi. p. 167.) And with regard to the subject of nubification, I would remark that the negative as well as the positive electricity of condensation must be equally considered.

I will now state some new experimental results at which I have arrived bearing on this subject. I find that saline matter, which when discharged with the steam readily extinguishes the ordinary frictional electricity of steam, does not prevent the boiler from becoming positive as before (70.), and that some water must escape with the steam in order that the boiler may become positive. I think these experiments will demand that we regard the positive state of the boiler and the negative state of the steam as produced by the friction of steam on the wetted surface of the orifice of the jet. I have also

ascertained that a current of steam passing through the tin pipe simultaneously with a current of drops of water produces no electricity of condensation, provided the air is not admitted at the same time. This result is, as far as the electricity of condensation is concerned, similar to those of (89.), in which the necessity of a good supply of air is fully seen.

I do not here give the details of the above experiments, being willing that they should form part of a paper which I hope shortly to communicate.

I am, Gentlemen,
Your very obedient Servant,
REUBEN PHILLIPS.

7 Prospect Place, Ball's Pond Road,
March 5, 1850.

XXXVIII. *On Quaternions; or on a New System of Imaginaries in Algebra.* *By* Sir WILLIAM ROWAN HAMILTON, *LL.D., M.R.I.A., F.R.A.S., Corresponding Member of the Institute of France, &c., Andrews' Professor of Astronomy in the University of Dublin, and Royal Astronomer of Ireland.*

[Continued from vol. xxxv. p. 204.]

88. THE writer desires to put on record, in this place, the following enunciations of one or two other theorems, out of many to which the quaternion analysis has conducted him, respecting the inscription of gauche polygons in surfaces of the second order; without *yet* entering on any fuller account of that analysis itself, than what is given or suggested in some of the preceding articles. See the Numbers of the Philosophical Magazine for August and September 1849. And in the first place he will here transcribe the memorandum of a communication, hitherto unprinted, which was sent by him, in the month last mentioned, to the Mathematical and Physical Section of the Meeting of the British Association at Birmingham.

89. Conceive that any rectilinear (but generally gauche) polygon of n sides, $\mathrm{BB}_1\mathrm{B}_2 \ldots \mathrm{B}_{n-1}$, has been inscribed in any surface of the second order; and that n fixed points, A_1, A_2, .. A_n, *not* on that surface, have been assumed on its n successive sides, namely A_1 on BB_1, A_2 on $\mathrm{B}_1\mathrm{B}_2$, &c. Take then at pleasure any point P upon the same surface, and draw the chords $\mathrm{PA}_1\mathrm{P}_1$, $\mathrm{P}_1\mathrm{A}_2\mathrm{P}_2$, .. $\mathrm{P}_{n-1}\mathrm{A}_n\mathrm{P}_n$, passing respectively through the n fixed points (A). Again, begin with P_n, and draw, through the same n points (A), n other successive chords, $\mathrm{P}_n\mathrm{A}_1\mathrm{P}_{n+1}$, $\mathrm{P}_{n+1}\mathrm{A}_2\mathrm{P}_{n+2}$, .. $\mathrm{P}_{2n-1}\mathrm{A}_n\mathrm{P}_{2n}$. Again begin with P_{2n}, and draw in like manner the n chords, $\mathrm{P}_{2n}\mathrm{A}_1\mathrm{P}_{2n+1}$, $\mathrm{P}_{2n+1}\mathrm{A}_2\mathrm{P}_{2n+2}$, .. $\mathrm{P}_{3n-1}\mathrm{A}_n\mathrm{P}_{3n}$. Then one or other of the two following

Theorems will hold good, according as the number n is *odd* or *even.*

Theorem I. If n be *odd*, and if we draw *two tangent planes* to the surface at the points P_n, P_{2n}, meeting the two new chords, PP_{2n}, $P_n P_{3n}$, respectively, in two new points, R, R'; then *the three points* BRR' *shall be situated on one straight line.*

Theorem II. If n be *even*, and if we describe *two pairs of plane conics on the surface*, each conic being determined by the condition of passing through *three points* thereon, as follows: the first pair of conics passing through BPP_{2n}, and $P_n P_{2n} P_{3n}$; and the second pair through $BP_n P_{3n}$ and $PP_n P_{2n}$; it will then be possible to trace, *on the same surface, two other plane conics*, of which *the first shall touch the two conics of the first pair, at the two points* B and P_n; while *the second new conic shall touch the two conics of the second pair, at the two points* B and P_{2n}.

90. With respect to the *first* of the two theorems thus communicated, it may be noticed now, that it gives an easy mode of resolving the following *Problem*, analogous to a celebrated problem in plane conics :—To find the *two* (real or imaginary) polygons, $BB_1B_2 \ldots B_{n-1}$ and $B'B'_1B'_2 \ldots B'_{n-1}$, with any given *odd* number n of sides, which can be inscribed in a given *surface* of the second order, so that their n successive sides, namely BB_1, B_1B_2, .. for one polygon, and $B'B'_1$, $B'_1B'_2$, .. for the other polygon thus inscribed, shall pass respectively through n given points A_1, A_2, .. A_n, which are not themselves situated upon the surface. For we have only to assume at pleasure *any* point P upon that surface, and to deduce thence the *two* non-superficial points lately called R and R', by the construction assigned in the theorem; since by then joining the two points thus found, *the joining line* RR' *will cut the given surface of the second order in the two* (real or imaginary) *points*, B, B', which are adapted to be, respectively, *the first corners of the two polygons required.*—That there *are* (in general) *two* such (real or imaginary) polygons, *when the number of sides is odd*, had been previously inferred by the writer, from the quaternion analysis which he employed. Indeed, it may have been perceived to be, through geometrical deformation, a consequence of what was stated in § XIV. of article 87 of this series of papers on Quaternions, for the particular case of the ellipsoid, in the Philosophical Magazine for September 1849. See also the account, in the Proceedings of the Royal Irish Academy, of the author's communication to that body, at the meeting of June 25th, 1849; in which account, indeed, will be found (among many others) both the theorems of the preceding article 89; the second of those theorems being however there enunciated under a *metric*, rather than under a *graphic* form.

[To be continued.]

XXXIX. *Proceedings of Learned Societies.*

ROYAL ASTRONOMICAL SOCIETY.

[Continued from p. 151.]

Jan. 11, 1850. ON the Phænomena attending the disappearance of Saturn's Ring. By the Rev. W. R. Dawes.

"The interesting phænomena attending the disappearance of Saturn's ring in 1848, can scarcely have failed to attract the attention of the possessors of powerful telescopes in this country; but none, I believe, of the observations made have been presented to the Astronomical Society. From Professor Bond, however, a paper appears in the Monthly Notices for November 1849, containing observations made on Saturn with Merz's large refractor at Cambridge, U.S.; and also some inferences which he deduces from them.

"My own refractor (then at Cranbrook) being employed on a regular series of micrometrical observations, was only occasionally turned on the planet. What was noticed, however, was usually recorded at the time in my observatory journal, though without any intention of publication; for it was imagined that the results of observations with far more powerful telescopes, reflecting and refracting, would doubtless appear in the Monthly Notices, and render my own superfluous. On a perusal of Professor Bond's paper, however, I find that I have noticed several interesting particulars not alluded to by him; and also that the deductions I have drawn from some of the phænomena observed by both of us differ greatly from his. I therefore beg to present to the Society the following extracts from my journal, with some remarks upon them.

"'1848, June 30. Saturn. Power 252. The *ansæ* of the ring are *not quite* invisible. They are of a *deep coppery tinge*; and on the following arm is a faint satellite. I get an occasional glimpse of a similar appearance on the preceding arm, but cannot decidedly verify it. The planet is more than $2\frac{1}{2}$ hours east of the meridian. On looking again carefully with powers 252 and 163, I cannot be certain of either of the points *upon* the ring; for the *ring itself* seems *brighter* in those parts; and I question whether it be not a small portion of the ring which reflects more light. On estimating carefully, I find the part must be about *the extremity of the inner ring*, which is its brightest part.

"'July 15. The ring is visible, *with the two dots* [bright points] *on it as before.*

"'August 9. Occasionally an exceedingly faint *dusky red* line extends on each side of the shadow on the ball [being the arms of the ring].

"'August 20. The shadow of the ring on the ball is very narrow. *The arms of the ring not visible.*

"'Sept. 1. Power 163. Excessively narrow shadow of the ring on the ball. Belts strong, especially the two parallel ones in the northern hemisphere. *No trace of the ring.*

"'Sept. 2. 11^h G.M.T. Power 163. *The ring is visible* as an

excessively narrow line *of nearly the same colour as the planet.* The Nautical Almanac gives *Sept.* 3 as the time of its reappearance. *No shadow* on the ball.

" 'Sept. 3. Power 188 [an equiconvex single lens]. The ring is bright, though rather duller in colour than the planet. There is a dusky line about the same degree of shade as the belts (which are strong to-night), crossing the ball precisely in the line of the ring. It is very different from the *shadow* of the ring. Is it the *ring itself*, which, being less bright than the planet, is visible on the ball; as Jupiter's third and fourth satellites are visible on his disc as they transit over it? Night fine. Enceladus very well seen, and watched nearly up to the preceding arm.

" 'Sept. 4. Ring much brighter than last night. The dusky shade across the ball is visible, but not so distinct as it was last night. The following arm appears decidedly longer than the preceding one.'

"A few days after the last observation, I went to Starfield on a visit to Mr. Lassell, and on every opportunity that offered observed Saturn with his powerful 20-foot reflector. The following are some of the memoranda entered in my journal.

" 'Sept. 12. $11^h 20^m$ M.T. The ring is the finest line imaginable, yet of a pretty clear bright planetary colour.

" 'Sept. 13. *No ring visible, nor shadow.*

" 'Sept. 14. No ring visible. There is a very narrow dusky line [on the ball] just south of the bright equatoreal region.

" 'Sept. 19. The ring is *just visible* when the planet is best seen.' [It was frequently noticed to-night during the series of observations which placed beyond doubt the satellite nature of Hyperion.]

"The subsequent observations were made after my return to Camden Lodge, Cranbrook, with my $8\frac{1}{2}$-foot achromatic.

" 'Oct. 6. No decided appearance of the ring on either side. $9^h 45^m$, Tethys has occulted Enceladus. The planet is exquisitely seen. The line across the ball is very sharply defined and black, though I thought not quite *uniformly* so; the northern edge appearing blacker than the southern. [The *obscure* surface is now turned towards the earth.]

" 'Oct. 11. The arms of the ring are *frequently visible*, especially that on the *following* side, which is far more steadily seen than the other.

" 'Oct. 26. Exquisite vision. Glimpses of a *division in the dark shade on the ball.*

" 'Oct. 30. I have several times this evening received the impression of the dark line across the ball being *divided by an excessively fine line of light*, perhaps a trifle north of its middle. Most frequently suspected with powers 323 and 460.

" '$11^h 30^m$. Enceladus is at the end of the following arm very nearly. There is a pretty bright dot *on* the following arm, and a similar one on the preceding arm *.

* The night of Oct. 30 was very fine. Enceladus was bright with power

" 'Nov. 21. Very fine night. Enceladus was seen the instant the object was brought into the field of the *double-image* micrometer, power 435; and when the image was divided, the two points were still distinctly visible; proving that the telescope will show a lucid point of *half the brightness of Enceladus*. The dark shade [on the ball] is occasionally seen *divided by a bright and excessively narrow line*. The planet bears 460 very well.

" 'Dec. 5. The ring is curiously broken into portions, which look almost like small satellites. Occasionally the planet is very well seen.

" 'Dec. 20. The ring is obvious enough, but is broken, especially on the following side.

" 'Dec. 21. The ring is reduced to a very fine line, occasionally appearing broken, but sometimes entire. The eastern arm is not quite so bright as the western.

" '1849, Jan. 6. The ring is scarcely ever visible, but occasionally in the best moments it may be traced nearly or perhaps quite to the extremity. But some portions of it are brighter than others.'

" At this time the earth was elevated about half a degree to the *north* above the plane of the ring; the sun being about 1°·8 to the *south* of that plane. On the 19th, the earth passed from the northern to the southern side of the plane of the ring; and the *bright* surface became visible. Cloudy weather precluded observations till the 22nd.

" 'Jan. 22, 7^h 12^m. Titan is in contact with the southern side of the following arm, and about two-thirds of its length from the edge of the planet. The *ring* is bright, though narrow. *It is very much brighter in two places similarly situated on each arm*; and they coincide with the extremities of the inner and outer ring.'

" Titan was distinguishable from the brighter parts noticed when observed at 7^h 12^m. It afterwards came into coincidence with those on the following arm as it moved along it."

Remarks.

" 1. From the observations above detailed, it is obvious that when the *obscure* side of the ring is turned towards the earth, it is not invisible with moderate optical power. It also appears that *its visibility diminished* as the earth approached its plane, and its edge, consequently, was turned directly towards us. This is proved by the increased *difficulty* of seeing it with my refractor on Aug. 9, compared with July 15; between which dates the minor axis of the ring had considerably diminished; by its *invisibility* on Aug. 20 and Sept. 1, when the earth was very nearly in the plane of the ring; and by the increasing *facility* with which it was observed between Oct. 11 and the end of November, during which interval the minor

460, and was easily watched nearly up to contact with the following arm of the ring. Its distance from the planet's centre was measured with the parallel-wire micrometer under slight illumination of the field. At 7^h 16^m 30^s G. M. T. the distance was 34″·40 by four observations. With 460 and 658 the two bright satellites of Uranus were steadily seen.

axis had increased. And it is still more conclusively shown by the *total invisibility* of the ring even with Mr. Lassell's 20-foot reflector on Sept. 13, when both the earth and the sun were very nearly in the plane of the ring, and consequently the sun's light would be reflected from its *edge* with scarcely any obliquity.

"Hence it may be inferred that the *edge* of the ring reflects extremely little light, and that the visibility of the ring, when its obscure side is turned towards the earth, does not arise from the sun's light reflected from the *edge*, but from a feeble illumination of the *obscure surface*; and that it is *this surface only* which is seen and not the edge.

"This conclusion is supported by the appearance of *bright points* in some parts of the ring when obscurely visible; those points remaining *stationary*, and not partaking of the rotation of the ring. That inequalities should exist on the *edge* of the ring so large as to appear like satellites, even with a moderate telescope, when the edge itself, though directly turned towards us and fully illuminated, was quite invisible with a reflector of 24 inches aperture (whose illuminating power is about equal to that of an achromatic refractor of 17 inches aperture), seems to be quite inadmissible.

"It may be inferred, therefore, that the appearance of bright points in the unilluminated ring arises from the greater reflective power of some portions of its *surface*; the exterior portion of the inner ring being usually the largest and brightest of all, and visible on both arms at equal distances from the ball. The brightness of these points was not always precisely the same on each side; at some times the eastern, and at other times the western, appearing the brighter. This may readily be accounted for by supposing that the ring, at a given distance from its centre, may not possess the same degree of reflective power throughout its whole circuit: indeed, it is scarcely probable that it should; and the observed fact is in perfect harmony with the *rotation* of the ring, which the stationary appearance of irregularities on the *edge* can scarcely be.

"2. The inquiry is interesting, *Whence is the light derived which renders the obscure surface of the ring visible?*

"On this subject Sir W. Herschel says (in a paper read before the Royal Society, Nov. 12, 1789), 'I may venture to say, that the ring cannot possibly disappear on account of its thinness; since, either from its edge, or the sides, even if it were square on the corners, it must always expose to our sight some part which is illuminated by the rays of the sun: and that this is plainly the case we may conclude from its being visible in any telescopes during the time when others of less light had lost it, and when evidently we were turned towards the unenlightened side; so that we must either see the rounded part of the enlightened edge, or else the reflexion of the light of Saturn upon the side of the darkened ring, as we see the reflected light of the earth on the dark part of the new moon. I will, however, not decide which of the two may be the case, especially as there are very strong reasons to induce us to think that *the edge of the ring is of such a nature as not to reflect much light.*'

"Relinquishing, then, the idea of the visibility of the mere edge of the ring (whether from its extreme thinness or its unreflective nature), it may be asked, 'Do the appearances warrant the conclusion, that the obscure surface is rendered visible by the reflexion to us of the light reflected upon it by Saturn?'

"If this were the case, might we not reasonably expect that its *colour* would be somewhat similar to that of the obscure portion of the moon as enlightened by the earth? It is, however, of a totally different hue, and strongly resembles the *tarnished copper* colour frequently assumed by the moon under a total eclipse. May it not also be fairly questioned whether the ring would be so brilliantly illuminated by the reflected light of Saturn (which must fall but feebly on the half of the ring furthest from the sun), as to cause small portions of it to rival in brightness the satellites themselves illuminated by the direct rays of the sun; the brightest points of the ring (the eastern and western extremities) receiving reflected light from *one half only* of the illuminated surface of Saturn, which would be seen from them as a *half-moon*?

"I would venture, therefore, to suggest that the illumination of the obscure surface of the ring arises from the *refraction of the sun's light through an atmosphere surrounding each of the rings*, and thus throwing a pretty strong *twilight* upon them. During the whole time that the obscure surface was turned towards the earth, the sun was not more than two degrees below its plane; and during the period embraced by the observations, the depression scarcely ever amounted to one degree. A very moderate density of atmosphere, therefore, might suffice to produce considerable illumination of the obscure surface; much greater, probably, than the reflected light of Saturn could give, and of a far *ruddier tinge*, more nearly resembling that of our western sky shortly after sunset.

"3. It was occasionally observed, and on Oct. 6 it is recorded, that the dark shade on the ball was not uniformly black, its *northern* portion being *blacker* than the southern. On three unusually fine nights, viz. Oct. 26, Oct. 30, and Nov. 21, this shade was seen to be divided through its whole length into two parts by an excessively fine bright line. After scrutinising the object for a long time with high powers, I remained perfectly convinced of the fact, which I can account for only by supposing that the northern and blacker portion was the shadow of the ring cast by the sun upon the ball; that the somewhat lighter shade was the ring itself seen projected upon Saturn; and that the bright line between the two was a portion of the equatoreal regions of the planet. The relative situations of the different bodies at the time renders, I think, such an explanation probable."

ROYAL INSTITUTION.

March 15, 1850.—The Astronomer Royal "On the present State and Prospects of the Science of Terrestrial Magnetism."

The Lecturer commenced with remarking, that the subject of his

lecture would not be the exhibition of new and successful experiments, but the indication of trains of scientific research, in which at present all is doubtful and difficult, and in which the only light which seems likely to guide us may possibly lead us in the wrong direction. He then pointed out the difference, as it appears to be usually understood, between *knowledge* and *science*, that the former of these terms implies only the collection and careful arrangement of accurately observed facts, while the latter implies in all cases the idea of causation, and usually a reference to mechanical causes of a simple kind, whose complexity of action depends upon the specialities of distance, mass, &c. of the bodies upon which they act. This distinction was illustrated by the state of astronomy, which before the time of Newton was merely a collection of empirical rules, and after that time became a science (the most perfect that is known) by reference of movements to gravitation as a mechanical cause; and by the theory of light, which before Fresnel's time was a collection of facts only, but after that time, when the facts were explained by undulations (which are necessarily the effect of mechanical laws), became a true science. The same distinction was applied to the collections of statistical facts and the science of political œconomy, the moral causes in this science being analogous to the mechanical causes in the physical sciences. In these cases it was not to be supposed, nor was it possible, that we had come to the first cause; every general cause to which we could refer might itself be the subject of a more general cause: it suffices for us that we have gone as far back as perhaps our nature permits. Applying these views to terrestrial magnetism, it was to be said that terrestrial magnetism is not at present a science; and the particular object of this lecture was to point out what efforts had been made to bring it to the state of a science, and in what direction we ought probably now to direct our efforts, and with what prospect of success.

Passing over the notorious fact of the direction of the magnetic needle, the lecturer showed, by simple experiments, that terrestrial magnetism is not an absolute, but a directive force (having no tendency to move the magnet bodily, either north or south), and that the poles of opposite nature of two magnets attract each other, and that the poles of similar nature repel each other: and he insisted on the advantage of using terms like *austral* and *boreal*, not too closely connected with north and south, to express the kinds of magnetism residing in the south and north poles of the earth, considered as a magnet, and in the north and south poles of a free needle. He then pointed out that the observation of the time of vibration of a magnet might be made subservient to the determination of the *proportion* of the magnitudes of the horizontal magnetic force at different points of the earth; and expressed his regret that the mathematical character of Gauss's beautiful and most valuable method for forming an *absolute* measure of the force, entirely independent of the magnet employed, prevented him from offering it to the audience. The dip or inclination of the needle was then described, and its general law (the

austral pole dipping in north magnetic latitudes, and the boreal pole dipping in south magnetic latitudes) was explained. It was also shown experimentally that this is generally analogous to what happens when a small magnet is subject to the action of a large one; and, theoretically, that this is a consequence of the attractions and repulsions of poles (the magnitude of the forces being inversely as the square of the distance,—a law which from various considerations is established with the utmost certainty). He adverted to the mechanical composition of forces, and showed that, from the dip and the horizontal force, the whole magnetic force might be found. And he stated that, on approaching the *magnetic poles* of the earth, namely, those points to which (as observed at no great distance) the needle converges on all sides, and on approaching which, the directive force becomes smaller to evanescence, and the dipping-needle dips more and more till it is vertical, the whole magnetic force does not diminish, but increases nearly to its maximum.

Then were pointed out the construction of Gauss's bifilar magnet, in which the magnet is strained to a position at right angles to that of the free magnet, by the torsion of position of its two suspending threads, and every change in the magnitude of the horizontal magnetic force is shown by the position of the magnet, which is pulled more or less in opposition to the torsion;—and the construction of Lloyd's vertical-force magnet, in which a magnetical bar, mounted like a scale-beam, is loaded to a position of horizontality, and every change of the vertical part of the magnetic force is shown by the position of the magnet, which is inclined more or less in opposition to the preponderating weight.

Allusion was then made to the organized system of observations of magnets at every five minutes of Göttingen time on certain days in the year (Sundays), first established by Gauss, with the assistance principally of students of the German universities, but afterwards extended to other parts of Europe; and to the enormous extension of the system, principally by the Russian and the British Governments and the East India Company, over every part of the world (the simultaneity of observations and the use of Göttingen time being retained throughout, but the days of the five-minute observations being changed from Sundays to week-days); and to the Magnetical Expeditions which, with the establishment of distant magnetic observatories, constitute a national enterprise inferior to nothing but the French expeditions of the last century, and to the perfection of the organization and the improvements in the instruments and the mode of using them, especially at sea, which has been introduced by Col. Sabine.

The lecturer then pointed out the general character of the results. As regards the mean or average determinations (omitting the slow or secular changes, and deferring for a moment the rapid changes), nearly all collectors of results for declination, from the time of Halley, had conceived the existence of four magnetic poles:—two (the Hudson's Bay pole and the Australian pole) having been nearly

reached by voyagers; and two (the Siberian pole and the Cape Horn pole) being only inferred from the convergence of the directions of the needle. It had, however, been shown by M. Gauss that a theoretical investigation which would give as one of its consequences this convergence of directions would also negative the existence of the supposed poles; and on the whole the lecturer expressed himself as now doubtful of the existence of those poles. He expressed his regret that an idea long ago explained by Prof. Christie had not been followed out, namely, of the preparation of charts showing the lines perpendicular to the direction of the needle. A chart of the entire magnetic force was exhibited; and the general fact of no dip near the equator, and increasing dips of the austral end of the needle in the north, and the boreal end in the south, was again mentioned.

The next class of facts mentioned was the diurnal variation:—that in northern latitudes the austral end of the horizontal needle points furthest to the east at about eight in the morning, and furthest to the west at about two in the afternoon. In southern latitudes the change is in the opposite direction; and in low latitudes, as at St. Helena (Col. Sabine), and on the Red Sea (M. d'Abbadie) the change has the north-latitude character during the north-latitude summer, and the opposite character during the opposite season. The horizontal and vertical forces generally increase from morning to evening.

The third class of facts was the momentary changes first brought to light by the observations of the German *Magnetische Verein* (above-mentioned). These had appeared to the lecturer so important, that, principally for the better recording of them, he had brought before the British Association the importance of recommending to the Government to hold out the prospect of pecuniary reward to the inventors of effective self-registering apparatus. The Government had liberally responded to the application, and the consequence was, that most beautiful and effective photographic self-registering apparatus, constructed respectively by Mr. Brooke and Mr. Ronalds, had been combined with the free magnet, the bifilar magnet, and the vertical-force magnet, and had now been brought into daily use at the Royal Observatory of Greenwich, and at Toronto; and that their use appeared likely to extend. The general fact exhibited by the five-minute observations and by the photograph record is this:—The changes of direction of the horizontal needle and of the horizontal and vertical forces are incessant, and as examined at any one place appear most capricious. But if compared at several neighbouring places, for instance not exceeding 500 miles apart, they are found to be exactly similar: if the distance be increased, the similarity diminishes; and if places be selected spread all over the globe, it is usually found that a large disturbance at one place is accompanied by large disturbances at the other places, in which however it is difficult to trace the relation of the contemporaneous movements at the different places. Diagrams exhibiting these phænomena were placed before the meeting. The lecturer pointed out

one instance as strikingly showing how these phænomena appeared to indicate a distinct localization of their cause. Thus there were two disturbances of horizontal force at five stations (Catharinenburg, St. Petersburg, Greenwich, Göttingen, Milan), occurring at an interval of about a quarter of an hour; one of them showed increase of force at all the stations, the other showed decrease at the two first-named stations and increase at the others ; it appeared evident here, that the cause of the first was exterior to Europe, and the cause of the second was within Europe.

The division of the subject to which the lecturer then came was the cause of these phænomena. He illustrated by a model Hansteen's conception of two large magnets within the earth, stating that he understood it to be put forward only as an imaginary construction, generally (but not very accurately) representing the facts, but not to be taken for a representation of a real state of things. He then adverted to Gauss's beautiful and general investigation of the effects of a magnetic earth, supposing that every part of it was magnetic in every conceivable variety of manner and degree ; and stated that, by proper adaptation of certain constants in this general theory (a theory which it is totally impossible to express in ordinary language), all the recorded observations of the mean positions of the magnets might be well represented. But M. Gauss had stated the following as one consequence of the theory :—Supposing that every part of the earth has equal magnetism in the most favourable direction for producing the known effects with the smallest expense of power, then the quantity of magnetism in one cubic metre of the earth is equal to the magnetism of eight of the best steel magnets weighing 1 lb. each. This, in the lecturer's opinion, made the whole theory difficult to be received.

Connected with the theory of general magnetism of the earth, is Canton's explanation of the diurnal inequality. He supposed that if there were, near the equator, two magnets in north and south positions, one more east and the other more west than England, the rising sun would heat the eastern magnet, and thus (by a law which applies to steel magnets) would diminish its magnetic power, and the effect of the western magnet would then turn the English needle to a position verging more to the north-west and south-east, until the two magnets were equally heated.

The lecturer then exhibited experimentally Œrsted's discovery, that a simple helix of wire, through which a galvanic current passes, possesses all the properties of a bar-magnet, its opposite ends exerting opposite effects upon one pole of a magnet, and these effects being reversed upon testing it on the other pole of the magnet. From this it followed naturally, that a model of a sphere surrounded by a spherical helix carrying a galvanic current would nearly represent the condition of magnetism upon the earth,—and Barlow's experiment to that effect was exhibited : and it was shown that its action on a free dipping-needle is generally similar to the earth's action. He then adverted to Lubeck's discovery—that the applica-

tion of heat to the point of junction of two different metals (as bismuth and antimony, or bismuth and copper) creates a galvanic action, as is shown by connecting wires with the two ends of the united metals and forming a circuit; and observed that here we seemed to have in nature a cause which might explain the origin of terrestrial magnetism. Attention was then called to the general similarity of Sabine's lines of equal magnetic intensity with Humboldt's lines of equal temperature, the lecturer remarking, that a much greater similarity would have been seen if he had been able to display a chart of lines perpendicular to the direction of horizontal magnetism, as proposed by Professor Christie. Allusion was then made to the very remarkable experiment by Professor Christie, in which a disc of bismuth being surrounded by a ring of copper, and heat being applied to the edge of the copper, an extraordinary amount of magnetism was developed; two poles, austral and boreal, being produced at certain points on one surface, and poles of opposite character (separated from these by the thickness of the bismuth only) on the opposite surface. Professor Christie had endeavoured to extend this experiment to the case of a spherical copper shell filled with bismuth, and heated generally at the equator, but more particularly at one point; and the results appeared as far as they went to correspond well with the state of terrestrial magnetism, but the difficulty of insuring a good union between the copper and the bismuth (a difficulty which perhaps might now be overcome by electrotyping) had made the results somewhat uncertain.

The lecturer then remarked that, for the advancement of the truly scientific part of this inquiry, it does not appear that we have need of any new Expeditions or of any further accumulation of observations made on the present plan. We have already vast collections of observations which will be useless till they are published, and which cannot be properly considered in a few years. But it is probable that the discussion of them will suggest new instruments of observation, more especially if (as is his own opinion) great importance shall be thought due to the small disturbances. Already he had thought that in every fixed observatory eye-observations ought to be abandoned, and photographic self-registration to be substituted for it: and he now thought that it would be necessary so to improve the magnets that they may be sensible to more rapid disturbances, and so to improve the photographic paper that a momentary beam of light may make an impression upon it. But any suggestions as to the course to be pursued in tracing the causes of magnetism must be guided by the opinion of the person who undertook the inquiry. The lecturer's own belief is, that thermo-electricity is the fundamental cause: and in this belief he expressed his opinion that the importance of experimental investigation of the laws of thermo-electric magnetism, where broad surfaces of different metals are in contact, is paramount to every other. An experimenter might commence with Christie's valuable experiments, and extend

them as the results should seem to guide him. Too great importance cannot be attached to experiments as distinguished from observations. Thus, it may truly be said that the discovery of gravitation is founded rather upon the experiments of Galileo and the purely mechanical deductions from them, than upon the observations of the planets, which alone would never have led to it. Still, however, our knowledge derived from experiment must be combined with the observations: and now the question is, with what class of observations shall we begin? Shall we attempt to explain the mean state of magnetism,—or the diurnal inequalities,—or the capricious inequalities? In the lecturer's opinion, this choice would depend very much upon our judgement whether all these were to be ascribed to causes of the same class or of different classes. He believed himself that the causes of all were the same, varied in their effects only by the specialities of the circumstances under which they acted. They do not differ more than a broken sea among rocks differs from a smooth swell in the open ocean;—they do not differ more than the trade wind, the monsoon, the land and sea breezes, the variable winds, and the tornadoes differ: yet no one doubts that these are due to general causes of the same kind, modified by assignable specialities of circumstance. If, then, it were supposed that the causes of these classes of magnetic phænomena were the same, the class upon which he would propose to fix is the capricious disturbances. In the other classes we probably see, at any one time, only the result of an infinity of combined operations; in this, we see nature acting under the simplest circumstances. The comparative minuteness and obscurity of these phænomena is no argument against their efficiency for the scientific inquiry. The general laws of ordinary light had been known for centuries to many persons: the facts of depolarization are not known to one in a million, and have not been computed by one in a hundred millions; yet it is upon these that the undulatory theory of light in general is founded. It would probably be found then, that the successful course would be to confine the examination to a single momentary disturbance which could be traced well through all the magnetic observatories in the world (the instance to which the attention of the audience had been previously directed, showed the absolute necessity of limiting the disturbance to the shortest time possible),—and with such lights as experiment could give to determine by some mathematical process the locality of the disturbing cause. But no rule could be given for the process to be used. This only we might predict with certainty, that the investigation would be long and troublesome; but such investigations are not without their redeeming pleasure, and no title in philosophy could be too high for him who should bring the investigation to a successful end.

XL. *Intelligence and Miscellaneous Articles.*

NOTICE OF A METEOR. BY J. WALLIS, ESQ.

To the Editors of the Philosophical Magazine and Journal.

338 Albany Road, Camberwell,
March 4, 1850.

GENTLEMEN,

ON Friday the 22nd of February, at thirteen minutes before midnight, I saw in the S.S.E. by S., at about the altitude of 15°, a large bright meteor, whose apparent diameter was equal to that of the moon. It was visible during three or four seconds, while describing about seven or eight degrees, nearly perpendicularly downward. Its globular form was well-defined, and the anterior portion remained permanently so, while the hinder portion separated into numerous fragments, leaving evanescent streaks of a dull red colour for several degrees on the sky. My eye was fortunately directed to the place where it originated, so that I traced the whole of its course. But for the presence of the moon, which was shining brilliantly at the time, its illuminating power would have been very considerable. It was intensely bright, of the electric blue colour. The form underwent some change in the vertical direction, being somewhat suddenly elongated into an egg-shaped figure, the more obtuse end foremost. The atmosphere being at the time free from cloud or vapour, would account for the light being very local and circumscribed. I presume that in physical constitution it must have been very much like the meteor recently described in the *Pictorial Times* by the Astronomer Royal. The moon in the gibbous form afforded me a ready object of comparison; its lustre was more vivid than the serene countenance of our ever-beautiful and admired satellite.

I can easily imagine all the changes assumed in its track as resulting from the rapid passage of a coherent liquid or gaseous body through a resisting medium. In perfect quiet I listened for more than a minute after its extinction, but heard no explosion. Indeed, from the manner in which it finally broke up, might be inferred rather the idea that the luminous fragments were left detached from the general mass by the diminution of atmospheric pressure immediately behind it, than from an inherent force of explosion; for by the composition of such force with that common to the whole body, the fragments would have diverged in a direction more laterally, or across its path. As we know not the distance of the meteor, we have no data from which to determine its velocity; but little doubt can exist that the compression of the air before it must have been very great. Mere spontaneous ignition of any aggregate of combustible materials would produce only a radial divergency of its elements; hence a progressive motion of the whole points to some extraneous source of attraction or impulse; and the velocity consequent to or resulting from this may, by condensation of the air before it, elicit the heat in which its ignition originates. And this elevation of temperature may effect decompositions among its elements, which are attended with the observed evolution of their usually brilliant light.

I remain, &c.,

JOHN WALLIS.

NOTE BY A. H. H. ON THE SPHEROIDAL STATE OF BODIES. COMMUNICATED BY W. R. H.

The following account, which does not seem to be generally known, may perhaps be of some interest in connexion with the discoveries of M. Boutigny and others relative to the spheroidal state of fluids. The statement is extracted from the Dublin Philosophical Journal (now out of print) for Feb.–November 1826, and appears to have some resemblance to that of Boutigny.

"INVENTIONS, 59.—*Limits to the power of steam.*—M. Clement states, that in the conversion of water into steam, there is a certain limit beyond which the elastic force of the steam cannot be increased, however intense the heat applied. When this mechanist visited England, he witnessed some experiments made by Mr. Perkins, who has been so much employed in the construction of high-pressure engines. Mr. Perkins, having procured a cast-iron boiler of very great strength, applied an intense heat for the production of steam, expecting a corresponding increase in the elasticity of the fluid. To his great surprise, however, he found that after a certain degree, the force of the steam, instead of increasing, diminished. M. Clement thinks that this unexpected result may be thus accounted for:—the steam, when submitted to intense heat, repels the remaining water from the internal surface of the vessel, and keeping it suspended at a short distance from the heated metal, interrupts the change into the elastic state. Thus, he observes, drops of water will remain unchanged for some time on the surface of red-hot iron; but if they be struck with a hammer, they are immediately converted into steam, exploding with considerable force. Thus, also, steam-engines may explode, though in good condition, and having the safety-valves acting freely. For if the temperature be lowered quickly after having been previously very high, the water, which according to this theory had been repelled from the internal surface of the boiler, is suddenly brought into contact with it, so that steam is produced in such quantities that it cannot pass off by the safety-valve, and causes the vessel to burst.—*Bullet. Un.* 1826, No. 3. sect. 6. p. 203."—*From the Dublin Philosophical Journal*, vol. ii. Feb.–November 1826, p. 433. Inventions, 59.

ANALYSIS OF A GRANULAR ALBITE. BY PROF. M. H. BOYE.

[In reprinting Prof. B. Silliman's, jun. Observations on some American Minerals (vol. xxxv. p. 484), we accidentally omitted a note appended to that gentleman's analysis of a granular albite associated with corundum from Westchester, Pennsylvania, in which he alludes to the previous analysis of the same mineral by Dr. Boye. Our attention has since been directed to this omission by Dr. Boye, who has kindly furnished us with the following further information copied from the Proceedings of the American Phil. Soc. vol. ii. p. 190, May 20, 1842.—ED.]

"M. Boye made an oral communication relative to a white cry-

stalline mineral, which occurs three or four miles to the south of Westchester, Pennsylvania, and which incloses corundum and several other mineral species.

"The specimen was handed to him for examination by Mr. Nuttall several years since, and proving to be a silicate closely allied to a felspar, he subjected it to analysis in conjunction with Prof. Booth, in order to compare it with the several felspars previously investigated by them.

"It forms a white translucent mass composed of densely aggregated crystalline grains, and might be mistaken, at the first glance, for a moderately coarse-grained marble, did not its hardness indicate a totally different substance. Its specific gravity is 2·612.

"The analysis was performed in the manner mentioned in the Proceedings of the Society for May 1841, and gave the following results:—

		Oxygen.		
Silica	67·72	35·18		
Alumina with a trace of iron	20·54	9·593		12·69
Magnesia	0·34	0·131	3·101	
Lime	0·78	0·219		
Soda	10·65	2·724		
Potassa	0·16	0·027		
	100·19			

"This composition approaches nearest to that of albite, excepting in a deficiency of silica, in which respect it resembles the albite from the vicinity of Wilmington, otherwise corresponding to it closely in composition; and agrees also with an albitic felspar from Pennsylvania, analysed by Redtenbacher in Prof. Rose's laboratory at Berlin (Poggendorff's *Annalen*, vol. lii. p. 469), as shown by the following comparative table.

Albite.	Six miles N.W. of Wilmington, Del. Booth and Boye.	Pennsylvania. Locality not stated. Redtenbacher.	Granular variety. Westchester, Pa. Booth and Boye.
Silica	65·46	67·20	67·72
Alumina	20·74	19·64	20·54
Sesquioxide of iron	0·54	..	a trace
Magnesia	0·74	0·31	0·34
Lime	0·71	1·44	0·78
Soda	9·98	9·91	10·65
Potassa	1·80	1·57	0·16
	99·97	100·07	100·19"

ON A SERIES OF INSOLUBLE SALTS OF PHOSPHORIC ACID. BY M. H. ROSE.

When organic substances are carbonized without the contact of air, and we then treat them with water till it ceases to dissolve anything, hydrochloric acid often extracts from the charcoal exhausted by water, a notable quantity of potash and soda and earthy phosphates; the

alkaline phosphates having formed with the earthy phosphates, double salts which are insoluble in water but soluble in acids.

Ammoniaco-magnesian phosphate is the only known double earthy phosphate insoluble in water. But double salts which are absolutely analogous may be formed with potash, soda, and even lithia, either with magnesia or lime.

When the phosphate of lime or magnesia is fused with an excess of an alkaline carbonate, a double salt is formed. But by this process it is not obtained pure, because the excess of the alkaline carbonate always effects partial decomposition. The higher the temperature is raised, the more complete is the fusion and the larger is the quantity of earthy phosphates decomposed. With an atom of phosphate of magnesia mixed with an atom of potash or soda, complete decomposition may be effected; but with phosphate of lime this is not the case; the cause is, that the decomposition of the double salt of the alkaline phosphate, and phosphate of lime, is effected with difficulty by the alkaline carbonate.

But when the earths are intimately mixed with a small proportion of alkaline carbonate and heated, so that the mass neither fuses nor sinks, all the carbonic acid is expelled from the alkali, and it is then not possible to extract the alkali completely from the calcined mass by water either cold or hot.

It is difficult to obtain this compound in a state of great purity. This depends partly on the quantity of carbonate of potash employed. If it be too small, the compound obtained is mixed with too much earthy phosphate; if too much be used, the decomposition is but slight, and the compound contains a larger or smaller proportion of earthy carbonate. The composition of the compound partly depends also upon the degree of washing, which modifies it more or less considerably.

Make a very intimate mixture of an atom of earthy pyrophosphate with an atom of an alkaline carbonate, and heat the mixture till it ceases to lose weight. The calcined mass is then neither fused nor sunk; it is to be heated for some time in water, and washed with it boiling.

The compounds obtained yield by analysis greater or less approximations to a combination of two atoms of earth, one atom of alkali and one atom of phosphoric acid; so that the proportion of oxygen of the bases approximates that of the phosphorus in the proportion of 3 to 5.

In the greater number of cases the washing requires much time, and in some it seems interminable; it appears, therefore, that the alkali is obstinately retained; but a large proportion, and frequently the greater part, may be separated from the earthy phosphate by prolonged washings.

In these cases, the alkali removed by washing is replaced by an equivalent of water; so that a compound, washed for a long time with hot water, is partly constituted of the original compound which is not decomposed, and partly of a new combination of two atoms of earth, one atom of water, and one atom of phosphoric acid, these compounds existing either mixed or chemically combined.

If excess of the alkaline carbonate be used in the preparation of the compound, and the earthy phosphate is fused, the washed compound which results is mixed with a greater or smaller proportion of earthy carbonate. The compounds which were prepared and analysed were the following:—

1. Phosphate of potash and lime ($KO+2CaO+PO^5$).
2. Phosphate of soda and lime ($NaO+2CaO+PO^5$).
3. Phosphate of potash and strontia ($KO+2SrO+PO^5$).
4. Phosphate of soda and strontia ($NaO+2SrO+PO^5$).

These salts are transformed, by washing, into a compound which nearly approaches the combination—

$$2(Na+SrO+PO^5)+(HO+2SrO+PO^5).$$

5. Phosphate of potash and barytes ($KO+2BaO+PO^5$).
6. Phosphate of soda and barytes ($NaO+2BaO+PO^5$).

These two combinations, especially that of potash, do not seem susceptible of being obtained pure. Among all the compounds of phosphate of soda with the alkaline earths, the phosphate of barytes is mostly, though not completely decomposed, by treatment with the alkaline carbonate; but on treatment with water, part of the alkali in the insoluble double salt is easily replaced by an equivalent of water.

7. Phosphate of potash and magnesia ($KO+2MgO+PO^5$).
8. Phosphate of soda and magnesia ($NaO+2MgO+PO^5$).

These two compounds should be washed with ammonia water; in spite of this precaution, however, a great part of the alkali, especially in the latter, is replaced by water.

9. Phosphate of lithia and lime ($LiO+2CaO+PO^5$).

If earthy pyrophosphates be calcined with the metallic chlorides of the alkalies, the chlorine is expelled from them by the intervention of moist air, in the form of hydrochloric acid gas, and compounds are formed analogous to those which have been described.

But if the earthy phosphates are contained at the same time as the alkalies in acid solutions, the first may be precipitated by ammonia, without the precipitate containing any fixed alkali. The compound described cannot therefore be formed in the humid way. The whole of the earthy phosphate is not precipitated, on account of the presence of the ammoniacal salt.

In certain circumstances there appeared also to be formed analogous combinations of phosphoric acid, which probably consist of two atoms of alkali, one atom of earth, and one atom of phosphoric acid. These compounds are soluble in water.

When an organic substance is carbonized, and the coaly mass is exhausted by water, the aqueous solution contains earthy phosphates (especially phosphate of lime), which separate on evaporation to dryness, and the dry residue is dissolved in water. The earthy phosphates are dissolved by the alkaline phosphates, and form double salts with them.

When pyrophosphate of soda is gently heated with an excess of earthy carbonate, or when an organic substance is exposed to sufficient heat to carbonize it, the aqueous solution of the calcined mass

contains phosphate of lime. To make this experiment, a little more than an atom, or preferably two atoms of pyrophosphate of soda, and one atom of carbonate of lime are employed. In the aqueous solution, the presence of lime may be discovered by oxalic acid. The phosphate of lime does not always separate so well by evaporating the aqueous solution to dryness, or by passing a current of carbonic acid gas into it, as by decomposing it by carbonate of soda and evaporation. The phosphate of lime eliminated does not contain any carbonate of lime after washing.

In this case there is evidently formed a double salt of phosphate of soda and phosphate of lime. The latter separates when a portion of the soda is converted in the solution into carbonate of soda.—*L'Institut*, Decembre 26, 1849.

ON INSOLUBLE ALKALINE AND EARTHY DOUBLE ARSENIATES. BY M. H. ROSE.

Arsenic acid forms with the alkalies and earths double insoluble salts, analogous to those of phosphoric acid. The earthy arseniates are always much more readily decomposed by heat and by the alkaline carbonates than the phosphates are; consequently, if an excess of an alkaline carbonate be employed, the result is a complete decomposition. But if merely an atom of earthy arseniate and an atom of alkaline carbonate be heated together, partial decomposition is soon produced without undergoing fusion. If the calcined mass be treated with water, some earthy carbonate mixed with the insoluble compound remains, in which by long treatment with water the alkali is replaced by an equivalent of water, as already stated with respect to the insoluble double phosphates.

There is therefore but little chance of success in preparing analogous insoluble double salts of arsenic, but they certainly exist; and what proves this is the circumstance, that the calcined mass, although the washing may be long continued, always contains some alkali.—*L'Institut*, Decembre 26, 1849.

ON THE DISCOVERY OF PLATINA IN THE ALPS. BY M. GUEYMARD.

The author is chief engineer and director of mines, and discovered in 1847, platina on the mountain of Chapeau, in the commune of Champoleon, in the valley of the Droc. It occurs in gray copper ore, which besides contains from some cases as much as 12 per cent. of silver, some antimony, lead, zinc, iron, a little arsenic and sulphur; the gangue is a mixture of dolomites, quartz and barytes.

In 1847 platina was found in the alluvion of Columbia, of the Ural, Brazil, and of St. Domingo; in the rocks of diorite of the high mountains of Columbia (Boussingault), and in the serpentines of the Ural (Leplay). The existence of this metal had not been discovered in other places, and never in the Alps. M. Gueymard states, that in 1847 he had performed more than a hundred examinations or analyses

in his laboratory; he was certain of the results which he obtained, but he says that he met with difficulties which required a hand more practised in manipulating than his to overcome. Some specimens yielded an ascertainable quantity, but these were the exceptions; at other times very clear indications of platina were obtained, but the quantity was not determinable. Of one hundred specimens more than eighty yielded none.

Being of opinion that the platina of Chapeau was not a solitary deposit of platina in the Alps, the author directed his researches to Saint-Arcy near the Mure (Isère), and in the bournonites, dolomites, and altered limestones of these mountains, he found platina. These bournonites were treated in the same way as the gray copper of the Chapeau, and similar results were obtained. Platina was sometimes found in 5 grammes of the mineral, while in other cases none could be discovered in 20 grammes; the metal is however more common at Saint-Arcy than at the Chapeau. A third locality is the Plan des Cavales on the Montagne des Rousses, in Oisans (Isère). These mountains are composed of protogenes, gneiss, talcose schists, limestones and dolomite.

At the Plan des Cavales there are many ancient workings anterior to the discovery of gunpowder. Among the rubbish there occur specimens of carbonate of copper of a foliated structure, containing as much as 50 per cent. of copper, and this carbonate, which is of a dirty green colour, yielded in two cases ascertainable quantities of platina; other examinations did not yield a trace, but the author states that he possessed but a small quantity of the mineral. Lastly, on the right bank of the Bens in Savoy, on the lands of Presles, there was found a fourth locality, on which gray and carbonate of copper, containing but little silver, gave traces of platina; it occurred in all the specimens, and in some of them its quantity was ascertainable, though with difficulty.

At the request of M. Gueymard, M. Ebelmen examined the specimens from these four localities, and he found platina in several of them, though in quantity too small for working. M. Vicat, jun., also found indications of the presence of rhodium, and the same observation was made by the author.—*Comptes Rendus*, 31 Decembre, 1849.

ANALYSIS OF TIN FROM THE MINES OF BANCA. BY M. MULDER.

This metal was found to consist of—

Tin	99·961
Iron	0·019
Lead	0·014
Copper	0·006
	100·00

It contains therefore only $\frac{4}{10,000}$ of foreign metals. It resulted from the examinations performed, that peroxide of tin consists of—

Oxygen	21·476
Tin	78·524
	100·000

These results give 731·23 as the atomic weight of tin. M. Berzelius gave in 1835 the number 735·296, which has been generally adopted.

It is, however, admitted that the analyses cited were not of a nature to supply the exact value of the atomic weight of tin, although they show with certainty that it requires some alteration.

To decide this question several experiments were made with all the precautions required by operations of this nature. One hundred parts of perfectly pure tin, reduced in various ways from its oxide, gave 127·56, 127·56 and 127·43 parts of peroxide of tin. The atomic weight calculated from the first two experiments, which gave the same numbers, is 725·7. These analyses were performed by M. Vlaanderen.

This result places tin in the series of metals, the atomic weight of which is expressed by a multiple of that of hydrogen; the number 725, which is $58 \times 12 \cdot 5$, may be hereafter adopted for the equivalent of tin, seeing that the difference of 0·7 in 725 cannot be detected by chemical analysis. The composition of peroxide of tin inferred is therefore—

Oxygen..............	21·62
Tin..................	78·38
	100·00

Journ. de Pharm. et de Chém., Janvier 1850.

ANALYSIS OF THE MUD OF THE NILE.

The first analysis was made by M. Lajonchere in the laboratory of M. Payen; it was supplied by M. D'Arcet, Jun. The specimen was in irregular masses, which were readily reduced to a fine powder; it was soft to the touch, and contained brilliant grains. It adhered slightly to the tongue, and had a sensibly saline and acrid taste. Its density was 2·5, nearly equal to that of various fertile soils. When calcined in the air it assumes a reddish-brown colour, loses weight by the evaporation of water and the combustion of organic matter; when calcined in close vessels, it yields alkaline vapours acting on test paper.

For the analysis 1·638 gr. of the original mud was taken, containing 1·541 gr. of dry. This quantity of matter furnished 3^{cc} of gas measured at 19°, and under a pressure of 0·7565, which correspond to 3·43 millims. of nitrogen for 1·638 of original matter, or 2·22 for 1000 of dry matter; the original matter contained therefore 2·10 of nitrogen in 1000. These numbers show that the mud of the Nile approaches very fertile lands.

The incineration of the matter dried at 105° C. *in vacuo* for three hours indicated 4·75 of organic matter in 100 parts, or 4·75 for 1000. This organic matter contains then 4·67 of nitrogen in 100 parts.

The matter soluble in water was extracted in order to ascertain the nitrogen which it contained; the results were as follow:—

Heated to 100° C. in a water-bath in a litre of water, 100 grms. of the matter gave a solution which, filtered and evaporated on a vapour-bath, left a residue weighing 0·866 gr., and contained, as ascertained by calcination, 23 per cent. of its weight of organic matter; 0·237 of dried matter, analysed by combustion with oxide of copper, gave a volume of gas equal to 5^{cc}, under a pressure 0·7608 millim. at 19°, which corresponds to 0·00573 gr. of nitrogen, or 0·0109 gr. for 0·866 gr. of soluble substance contained in 100 grms. of dried matter.

The matter of this second analysis thus contains 2·19 per cent. of nitrogen, and the organic matter contained in 1000 of the dry mud represents 1·7 of nitrogen.

Lastly, it may be concluded from this experiment, that the organic matter of the mud of Egypt represents the whole of the nitrogen contained in this mud.

Mineral Analysis.

Portion soluble in water.	Silica	0·05
	Water	4·75
	Organic matter	4·85
	Alkaline chlorides	0·65
Portion soluble in hydrochloric acid.	Peroxide of iron	11·90
	Alumina	21·65
	Carbonate of lime	3·85
	Carbonate of magnesia	2·05
Matters insoluble in water and acid.	Silica	46·55
	Alumina	3·70
		100·00

Second Analysis of the Mud of the Nile, by MM. Payen and Poinsot. —The specimen was given by MM. Brongniart and Decaisne. This specimen had the form of a fine powder containing scales of yellow mica; when mixed with water it absorbs a certain quantity of it, and forms a paste possessing some plasticity.

When calcined in close vessels, it immediately yields alkaline vapours; when calcined in the air it leaves a reddish residue.

Its composition in 100 parts was found to be—

Water	3·25
Organic matter soluble in water	0·35
Organic matter insoluble in water	4·46
Alkaline chlorides	0·07
Sulphate of lime	0·37
Carbonate of lime	6·33
Carbonate of magnesia and magnesia	4·09
Silica	54·27
Alumina	10·77
Peroxide of iron	13·18
Lime	2·86
	100·00

It is very remarkable that the mud of the Nile contains no trace of phosphate. Regnault in 1842, and M. Lassaigne in 1844, did not find any.—*Journ. de Pharm. et de Chém.*, Janvier 1850.

PANOPTICON OF SCIENCE AND ART.

An advertisement on the Cover of our present Number announces the establishment of a new Chartered Institution on an extensive scale, and recommended by powerful patronage, the object of which will be the permanent exhibition of working models of machinery, and specimens of manufactures and fine arts, together with lectures on the several branches of natural philosophy and the arts; thus blending recreation with practical instruction. It is proposed that the institution, which is styled the Royal Panopticon of Science and Art, should be established by an association of chartered shareholders, and in a building to be erected for the purpose in a central situation near the Strand.

METEOROLOGICAL OBSERVATIONS FOR FEB. 1850.

Chiswick.—February 1. Densely clouded: showery. 2. Slight rain. 3. Cloudy: clear. 4. Very fine. 5. Slight rain: very fine: showery. 6. Boisterous. 7. Fine. 8. Hazy. 9. Very boisterous. 10. Clear: very fine. 11. Rain: boisterous. 12. Overcast: boisterous. 13. Clear and dry: frosty at night. 14. Rain: drizzly. 15. Cloudy: rain. 16. Clear. 17. Overcast. 18. Cloudy: very fine. 19. Overcast. 20. Densely overcast: rain. 21. Overcast: clear at night. 22. Cloudy: very fine. 23. Overcast and fine. 24. Overcast. 25. Foggy. 26. Foggy: overcast: clear. 27. Foggy: very fine: clear. 28. Foggy.

Mean temperature of the month	42°·80
Mean temperature of Feb. 1849	41 ·35
Mean temperature of Feb. for the last twenty-three years.	39 ·56
Average amount of rain in Feb.	1·61 inch.

Boston.—Feb. 1. Cloudy. 2—4. Fine. 5. Cloudy: stormy P.M.: distant lightning. 6. Cloudy: stormy A.M. 7. Fine. 8. Cloudy. 9. Fine: rain P.M. 10. Fine. 11. Cloudy: rain A.M. and P.M. 12. Cloudy: rain and snow P.M. 13. Fine: rain P.M. 14. Cloudy: rain A.M. 15. Fine. 16. Fine: rain A.M. 17. Fine. 18. Cloudy. 19. Fine. 20, 21. Cloudy. 22. Fine. 23—28. Cloudy.

Applegarth Manse, Dumfries-shire.—Feb. 1. Heavy rain nearly all day. 2. Rain all day: storm P.M. 3. Rain, with high wind. 4. Dull and cloudy. 5. Dull A.M.: severe storm of wind and rain. 6. Hurricane, with heavy rain. 7. Snow $\frac{2}{10}$ths of an inch deep: rain P.M. 8. Frost early: rain and wind P.M. 9. Rain and high wind all day. 10. Fine: frost A.M.: showery P.M. 11. Snow: rain: wind. 12. Rather fine: slight shower. 13. Hard frost: clear and fine. 14. Rain thick and close. 15. Rain: stormy P.M.: very wet. 16. Showers: very changeable. 17, 18. Wet A.M.: dull and moist all day. 19. Rain, and fog and wind. 20. Showers, short but severe. 21. Rain: storm of wind. 22. Fair, but unsettled-looking. 23. Fair: cloudy. 24. Fair and fine. 25. Rain during the night: moist. 26. Fine A.M.: showery P.M. 27, 28. Dull, but fair and mild.

Mean temperature of the month	41°·7
Mean temperature of Feb. 1849	41 ·2
Mean temperature of Feb. for the last twenty-eight years.	37 ·6
Average amount of rain in Feb. for the last twenty years.	2·04 inches.

Sandwick Manse, Orkney.—Feb. 1. Cloudy: rain: cloudy. 2. Showers: thunder: cloudy. 3. Showers. 4. Sleet: cloudy. 5. Cloudy: sleet-showers. 6. Showers: clear: aurora. 7. Showers: clear. 8. Cloudy: frost: sleet-showers. 9. Snow-showers. 10. Bright: snow-showers. 11. Cloudy: clear. 12. Bright: snow-showers: aurora. 13. Snow-showers: cloudy: aurora. 14. Rain: drizzle. 15. Cloudy: hazy: aurora. 16. Sleet-showers: hazy. 17. Rain: drizzle. 18. Hazy: drizzle. 19. Rain: hazy: cloudy. 20, 21. Sleet-showers: showers. 22. Cloudy: showers. 23. Cloudy: hazy. 24. Drizzle: fine. 25. Cloudy: fine. 26. Clear: fine. 27. Drizzle: cloudy. 28. Cloudy: fine.

Meteorological Observations made by Mr. Thompson *at the Garden of the Horticultural Society at* Chiswick, *near London; by* Mr. Veall, *at* Boston; *by the* Rev. W. Dunbar, *at Applegarth Manse*, Dumfries-shire; *and by the* Rev. C. Clouston, *at Sandwick Manse*, Orkney.

Days of Month.	Barometer.							Thermometer.								Wind.				Rain.			
	Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.
1850. Feb.	Max.	Min.		9 a.m.	9 p.m.	9¼ a.m.	8½ p.m.	Max.	Min.		Max.	Min.	9¼ a.m.	8½ p.m.								
1.	29·799	29·715	29·36	29·37	29·25	29·22	29·05	55	47	44	48	41	40½	43	sw.	sw.	w.	sw.	·02	·20	......	·36
2.	29·816	29·727	29·36	29·40	29·10	29·07	29·11	56	41	43	47	41½	46½	43	sw.	sw.	sw.	w.	·01	......	......	·43
3.	29·933	29·821	29·40	29·39	29·58	29·17	29·40	57	25	42	47½	39	42	43	w.	wsw.	sw.	wnw.	......	......	......	·17
☾ 4.	29·975	29·795	29·58	29·68	29·45	29·55	29·44	49	36	34	45½	35	41½	38	sw.	w.	sw.	sse.	......	......	......	·27
5.	29·634	28·977	29·26	29·39	28·50	29·14	28·18	49	39	42·5	46	33	37	40	sw.	wsw.	sw.-s.	w.	·16	......	......	·08
6.	29·250	28·911	28·36	28·51	28·95	28·47	28·83	46	32	43	42	35	40	38	nw.	w.	w.	wnw.	......	......	......	·30
7.	29·670	29·454	29·06	29·19	29·39	29·00	29·27	46	31	34	40	31	37	36½	w.	w.	wnw.	wnw.	......	......	1·30	·07
8.	29·722	29·675	29·33	29·26	29·20	29·18	29·04	51	39	39	48	31½	36	41	w.	s.	se-sw.	w.	......	......	......	·01
9.	29·699	29·428	29·05	28·90	29·10	28·83	29·04	50	35	49	44	43	36½	37½	sw.	s.	s–nw.	n.	·10	......	......	·28
10.	30·085	29·919	29·57	29·61	29·72	29·54	29·57	48	31	39	42	32½	36	40	w.	w.	w.	w.	......	·20	......	·30
11.	29·927	29·412	29·50	29·42	29·28	29·34	29·14	48	33	39	40	34	38	36½	sw.	sw.	w.	nw.	·11	......	......	·11
● 12.	29·404	29·254	29·00	29·03	29·19	29·08	29·26	45	29	34	40	31½	36½	32	w.	s.	s.	n.	·04	·29	......	·09
13.	30·236	29·837	29·57	29·80	29·90	29·84	29·69	43	21	32	36	27	31½	34	n.	nw.	ne-w.	s.	......	·07	......	·01
14.	30·126	29·957	29·63	29·58	29·60	29·32	29·42	51	47	35	48½	33	45	44½	sw.	ssw.	s–sw.	w.	·16	·04	1·55	·42
15.	30·007	29·952	29·55	29·56	29·40	29·24	29·29	56	43	51	50	46	47	43	sw.	w.	s.	wsw.	·14	·07	......	·02
16.	30·296	29·889	29·47	29·60	29·98	29·43	29·74	50	32	43	50	38	37	41½	nw.	w.	w.	w.	......	·04	......	·25
17.	30·300	30·209	29·76	29·90	29·90	29·54	29·57	50	42	43	48	40	45½	47	sw.	w.	w.	w.	......	......	......	·34
18.	30·199	30·146	29·66	29·85	29·82	29·50	29·61	54	43	47	49	45	48½	44½	sw.	w.	sw.	w.	......	......	......	·17
☽ 19.	30·080	30·008	29·65	29·70	29·51	29·41	29·14	50	48	44	46½	42½	50	50	sw.	sw.	sw.	sw.	·01	......	......	·49
20.	30·143	29·999	29·50	29·68	29·78	29·45	29·36	51	30	51	48	42½	41	41	sw.	w.	w.	wsw.	·20	......	......	·08
21.	30·165	30·138	29·59	29·60	29·76	29·23	29·58	50	44	47	45½	39	35	37	sw.	wsw.	w.	nw.	......	......	0·90	·53
22.	30·316	30·254	29·88	30·04	30·14	29·96	30·04	51	28	48	50	47	40	42	nw.	w.	w.	w.	......	......	......	·02
23.	30·305	30·236	29·88	30·08	30·02	29·90	29·93	56	32	44	47	42	45	45	w.	w.	wsw.	wsw.	......	......	......	·10
24.	30·289	30·275	29·90	30·10	30·11	30·06	30·13	42	33	43·5	47	39½	44	44½	sw.	w.	s.	sw.	......	......	......	·03
25.	30·366	30·349	30·00	30·13	30·13	30·08	30·03	48	36	42	45	39½	44	47	e.	s.	ssw.	s.	......	......	......	......
○ 26.	30·374	30·328	29·98	30·15	30·06	30·03	29·92	50	30	41	49½	39½	50½	47½	sw.	ssw.	ssw.	s.	......	......	......	......
27.	30·283	30·174	29·90	30·04	30·00	29·90	30·02	52	27	37	48	44	49	47	se.	w.	ssw.	sw.	......	......	......	·05
28.	30·202	30·141	29·80	29·99	30·00	29·97	29·83	52	37	41	46	41½	49	43	e.	calm	ssw.	s.	......	......	0·02	......
Mean.	30·021	29·857	29·52	29·606	29·600	29·444	29·451	50·21	35·39	42·0	46·0	38·3	41·76	42·03					0·95	0·91	3·77	4·98

THE

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PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[THIRD SERIES.]

MAY 1850.

XLI. *Observations on a remarkable Exudation of Ice from the Stems of Vegetables, and on a singular Protrusion of Icy Columns from certain kinds of Earth during frosty weather. By* JOHN LECONTE, *M.D., Professor of Natural Philosophy and Chemistry in the University of Georgia*.*

IT is certainly a remarkable circumstance, that phænomena so striking as those forming the subject of this paper have received so little attention from philosophers; and it is perhaps still more singular, that hitherto no attempts have been made at their explanation. Stephen Elliott, in his Sketch of the Botany of South Carolina and Georgia, published in 1824, notices a remarkable protrusion of crystalline fibres of ice from the stems of the *Conyza bifrons* (vol. ii. p. 322). Sir John F. W. Herschel published a short notice of a similar exudation of icy fringes occurring around thistle-stalks and stumps of heliotropes, in the London and Edinburgh Philosophical Magazine for February 1833, p. 110 (3rd series, vol. ii. p. 110)†. Professor S. P. Rigaud of Oxford notices the occurrence of an analogous phænomenon on a recently built stone wall, in the succeeding Number of the same Journal (3rd series, vol. ii. p. 190. 1833). As far as my researches extend, the above-mentioned notices—all of them very brief and imperfect—embrace all the observations hitherto made on these remarkable phænomena. Even the natural speculative tendency has been

* Communicated by the Author, having been read before the American Association for the Advancement of Science, at their extra meeting at Charleston, South Carolina, March 12, 1850.

† James D. Dana appears to have noticed the same phænomenon. He says, " On the cool mornings of spring or autumn in this climate, twigs of plants are occasionally found encircled by fibrous icy curls, which are attached vertically to the stem.' (Vide Manual of Mineralogy, 2nd Ed. p. 46. New Haven and Philadelphia, 1849.)

held in check by the extreme paucity of facts and observations, so that no explanation of them has been advanced.

For many years my attention has been drawn to the remarkable deposition of ice around the stalks of certain plants, as well as to analogous phænomena exhibited by certain kinds of soil. During a visit to the sea-coast of Georgia in the months of November and December 1848, I had a very favourable opportunity of studying the phænomenon as exhibited in vegetables. The plants in which I have observed it are two species of the genus *Pluchea* of DeCandolle, or *Conyza* of the older botanists, viz. *Pluchea bifrons* and *P. camphorata*. It is more common and conspicuous in the former species than the latter. Both of these plants grow abundantly in wet soils, around ponds and along the road-side ditches in the low country of Carolina and Georgia. The root is perennial, but the stem is annual and herbaceous.

The exudations of ice are most abundant and striking during the first clear frosty weather in November and December. At this period the earth is warm; and the serenity of the atmosphere is so favourable to radiation, that there is a remarkable difference between the temperature of the day and night. When the temperature sinks towards daylight to about 30° or 28° of Fahrenheit, or even lower, the surface of the ground is totally devoid of the slightest incrusting film of frozen earth, while hoar-frost is deposited in such profusion on all dead vegetable matter as to resemble a slight fall of snow. Under such circumstances, the traveller who passes along the level roads of this region soon after sunrise cannot fail to be struck with the remarkable accumulations of voluminous friable masses of semi-pellucid ice around the footstalks of the *Pluchea* which grow along the road-side ditches. At a distance they present an appearance resembling locks of cotton-wool, varying from four to five inches in diameter, placed around the roots of the plants; and when numerous, the effect is striking and beautiful.

In relation to the exudation of ice from the stems of vegetables, the description and delineation given by Sir John Herschel are so clear and faithful, and accord so exactly with the results of my own observations, that I prefer using the language of that justly distinguished philosopher whenever it suits my purpose. The engraving which accompanies his paper represents the appearances presented very accurately*. My observations appear to establish the following facts in relation to this phænomenon:—

* The reader is requested to refer to the engraving given by Sir John Herschel in the Philosophical Magazine for February 1833.

1. The depositions of ice are entirely confined to the immediate neighbourhood of the roots of the plants, the upper parts of the tall unbroken stalks being quite free from them. They frequently commence two or three inches from the ground, and extend from three to four inches along the axis of the stem. It is proper to state, that at this season the stalks are dead and quite dry to within about six inches of the earth, below which they are generally green and succulent. The plant has a large and porous pith, which is always saturated with moisture as high as six or seven inches from the base of the stem.

2. The ice emanates in a kind of riband- or frill-shaped wavy friable semi-pellucid excrescence, "as if protruded in a soft state from the stem, from longitudinal fissures in its sides." As Sir John Herschel remarks, "The structure of the ribands is fibrous, like that of the fibrous variety of gypsum, presenting a glossy silky surface, the direction of the fibres being at right angles to the stem, or horizontal." According to my observations, the number of ribands vary from 1 to 5. All of them issue from the stem in vertical or longitudinal lines, which are not always symmetrically disposed around the axis. Judging from the engraving given by Sir John Herschel, the *Pluchea* exhibits the phænomenon much more conspicuously and beautifully than the stumps of heliotropes observed by him. I have frequently observed the icy excrescences to exceed five inches in length; and when thus elongated, they are usually curled; often so much so, as to bring the remote extremity of the frill nearly in contact with its line of attachment to the stalk.

3. "Although," as Sir John Herschel correctly observes, "the icy sheets appeared to have been protruded from the interior of the stem, yet on examination they were found to terminate sharply at its surface, adhering to it so lightly as to render it impossible to handle a specimen without detaching them, and in no instance connected with any formation of ice within; on the contrary, the majority of the stems were sound and solid, and many of them still green when cut. The point of attachment of the ice was, however, always on the surface of the *wood*, beneath the outer bark or epidermis, which the frozen sheets had in every instance stripped off and forced out to a distance. Where the fringes were large and well-developed, the bark had quite fallen off; but in those cases where it adhered more strongly, it seemed to have prevented their free expansion; and in such instances the stem presented the singular appearance of a thick massive coating of ice interposed between the wood and its integument, which was

swollen and rifted." To the foregoing very accurate description I have only to add, that, according to my observations on the *Pluchea*, when the frost is quite *severe*, the icy sheets *were* often "connected with the formation of ice within," in fact, were continuous with the frozen pith; but under such circumstances the wood was always rifted longitudinally, and the process of protrusion seemed to have been completely checked at the part of the stem in which this took place. Indeed, the phænomenon was seldom exhibited in its most perfect and beautiful form when the wood was split. It is obvious, therefore, that in these instances the frigorific action was too intense to permit the phænomenon to be developed in a normal manner.

4. The phænomenon took place in the *same plant* during several consecutive nights; and when the wood was *not* rifted, frequently from the *same portion* of the stalk. When the wood *was* split, however, the deposition of ice occurred lower down the stem, at a part which was unaffected by the frost of the previous night. The stalks thus became completely rifted by a succession of severe nights, from the height of six or seven inches down to the ground. This is unquestionably one of the reasons why these exudations of ice are seldom observed after the middle of winter, for the stalks are usually destroyed before this period.

5. The stems which had been cut off within three or four inches of the earth exhibited the phænomenon as conspicuously as those which were left untouched. The icy sheets *never* issued from the *cut surface*, but *always* from *longitudinal lines* commencing somewhat below it and extending towards the root. Plants which were torn up and transplanted in a box of moist earth in a flower garden exhibited the same phænomenon, although much less strikingly than when left *in situ*.

"The appearances above described," to use the language of Sir John Herschel, "are quite at variance with any idea of the deposition of these icy fringes from the store of aqueous vapour in the general atmosphere, in the manner of hoarfrost; and the only quarter to which we can look for their origin is in the plant itself, or in the comparatively warm earth beneath, to whose exhalations the decaying stems may form a kind of chimney." The additional facts which my observations establish,—particularly in relation to the recurrence of the phænomenon on the same portion of the stalk during several successive frosts, even after it had been cut off, —appear to be irreconcileable with the idea that the physiological functions of the plant have any share in the production of it. We must therefore look to the moist earth for the large

supply of water necessary for the development of these voluminous masses of ice. But by what force and through what agency is it elevated and protruded?

Impressed with the idea that the phænomenon is purely *physical*, having no connexion with the vitality of the stem, it seemed reasonable that the remarkable exudation of *icy columns* from certain kinds of earth, which long attracted my attention, might be referred to a similar cause. Considerations of this character induced me to study the latter phænomenon more carefully. During the winters of 1848–49 and 1849–50, abundant opportunities occurred of examining the phænomenon under the most diversified circumstances, the soil in this neighbourhood being peculiarly adapted to its development.

The following facts seem to be established by my observations:—

1. The phænomenon occurs most strikingly when a warm rainy period terminates in clear freezing weather, with the wind from the west or north-west. It is more or less distinctly developed at all temperatures below 30° Fahrenheit. When, however, the thermometer was as high as this at sunrise, it was exhibited only in situations most favourable to radiation. It frequently appears during several consecutive nights after a rain; but usually, when the temperature remains nearly constant, with decreasing conspicuousness. This obviously arises from the diminution of moisture: in situations which are persistently wet, it is always developed in proportion to the depression of temperature.

2. It takes place in soils that are rather firm, but not very compact. For example, the phænomenon is beautifully exhibited along the sides of the water-worn ravines which furrow the declivities of the firm red clay hills of this primitive region, as well as along the cuts or ditches by the road-side. This clay seems to be formed by the decomposition, *in situ*, of hornblendic gneiss and mica-schist. This soil presents the same phænomenon when thrown up and lying on the surface, provided it is not trodden down and rendered too compact. For this reason it never appears on the well-beaten high roads, although it is seen abundantly along their margins. The influence of compactness of soil is strikingly illustrated by the fact, that the protrusion of the icy columns will frequently occur around the margins and along the middle cleft of a track of a cloven-footed animal, while none were found on the portions where the clay had undergone compression. The clods found at the bottom of the ravines and along the margins of the brooks generally afford beautiful manifestations of the

phænomenon, under proper circumstances. It is seldom, if ever, observed in rich, mellow, alluvial soils, abounding in vegetable matter.

3. The general appearance of the phænomenon is that of a vast number of filaments of ice, forming in their aggregation fibrous columns resembling bundles of spun glass, emanating at right angles to the surface, as if protruded in a semi-fluid state from an infinitude of capillary tubes in the ground. The structure of the columns is distinctly fibrous, presenting a fine silky, wavy, silvery surface, analogous to that of the fibrous variety of gypsum. They exhibit various degrees of diaphaneity, apparently depending on the purity of the water as well as on the state of aggregation of the icy filaments; being in some situations almost perfectly transparent, and in others scarcely semi-pellucid. Sometimes the fibres composing the columns are readily separable; at other times they are, as it were, fused together. When examined by transmitted light, *transverse striæ* are observed to cross the filaments at intervals varying from $\frac{1}{10}$th to $\frac{1}{30}$th of an inch. A thin stratum or crust of loose frozen earth is frequently detached and elevated on the summits of the columns, often forming a continuous roof-like covering to the soil beneath, extending over many square yards; at other times appearing in separate and isolated flat caps of variable size. The columns are not always uniformly distributed over the surface of the ground, but frequently exhibit considerable intervals of unfrozen soil between them. When the exudation takes place around the margins of a circumscribed depression containing water, like that left by the foot of a horse, it appears to draw up the water from the cavity, leaving an interior grotto lined with fantastic groups of icicles. The icy columns vary in length from one to three, four, or even five inches, according to the favourableness of the situation and the intensity of the cold. They vary in size from mere threads to prismatic bundles of one-fourth of an inch in diameter. When very long they frequently fall over by their gravity, presenting a beautiful appearance when viewed in masses. The effect produced by walking over a surface on which the phænomenon is well-developed is very striking. The superior crust of frozen earth and its supporting icy columns give way under the foot, which thereby sinks several inches below the apparent surface at every step. When the phænomenon occurs along the precipitous sides of the ravines and road-side cuts, the earth which has been elevated falls down to the bottom of the inclined plane as soon as the sun takes effect, leaving a fresh surface of soil exposed to the action of the next frost; and as this ex-

foliation continues from night to night when the weather is sufficiently cold, while all the earthy matter which is thus thrown down is carried off by the first considerable fall of rain, it is sufficiently obvious that it is a powerful agent of disintegration. When the weather is not severe, it is only exhibited in situations most favourable to the production of cold. The presence of a twig or a straw on the surface of the clay will, under these circumstances, determine the place of development of the phænomenon; and a twig will thereby be elevated above the general surface, supported by an elegant pectinated arrangement of icy columns.

4. On examination the icy columns were found to terminate sharply at the surface of the clay, adhering so lightly as to be detached by a mere touch of the finger, and *scarcely ever* connected with any formation of ice below,—in fact, *never*, under the circumstances most favourable to the development of the phænomenon. On the contrary, in the majority of cases, the soil from which they protruded *was not frozen* in the slightest degree, even during our severest weather, and when the earth in other situations was completely incrusted. This point was carefully examined early in the morning on the 11th of January 1849, when the thermometer was at 14° of Fahrenheit at sunrise; again on the 17th of February, when it was at 12°; and again on the 19th of the same month, when it stood as low as 5°,—a most extraordinary degree of cold for this latitude (34° N. lat.). These observations were carefully repeated on the mornings of the 4th, 5th, 6th, and 7th of February 1850, when the temperature was respectively 16°, 14°, 18°, and 23° of Fahrenheit's scale at sunrise. On *none* of these occasions was the ground, where the icy columns were developed in profusion, *frozen* in the slightest degree. The afternoon of February 4, 1850, afforded me the rare opportunity of observing the phænomenon in the very act of development. It took place on an eastern exposure at $5\frac{1}{2}$ P.M., when the temperature was 28° F. As the day was very cold, the icy columns of the previous night, which were about three inches in length, had been only partially melted, in this protected situation, by the influence of the mid-day sun. At the time the observation was made, these columns were found to be elevated about *one inch* by the recently protruded ice. The line of demarcation between the *old* and *new* ice-formation was perfectly distinct, the lower portions of the *former* having been remarkably attenuated by the process of liquefaction during the heat of the day. In this case it was obvious that the evolution of the phænomenon during the previous night and morning had been temporarily checked by the solar

heat, but was resumed as soon as that influence was withdrawn. The state of the soil was carefully examined; for it seemed to be almost certain, that the process of formation must have been going on under the eye at the time the observations were made. The subjacent clay was found to be moist and unfrozen, the icy columns separated from it with the slightest touch, and *were not* connected with any formation of ice below. As already intimated, in less favourable situations, when the frigorific action was intense, the soil on which the columns rested sometimes became incrusted with ice, after the protrusion had commenced; but this was invariably attended with a complete arrestation of the process: indeed, under such circumstances, it was obvious that there had been an imperfect development of the phænomenon. In these cases, a stratum of frozen earth was found adhering to the bases of the columns, while continuous icy threads were observed to transpierce this crust perpendicularly, and occasionally to extend into minute apertures in the unfrozen soil beneath it. As already remarked, in more favourable situations the ground beneath was never frozen; but on cautiously removing the icy columns, the moist clay was found to present a very porous appearance, as if perforated by a multitude of holes or spiracles, corresponding in position with the bundles of thread-like ice, and which were frequently of sufficient size to be quite obvious to the unassisted eye.

Having thus described with sufficient fullness the phænomena attending the occurrence of exudations of icy fringes from the stems of plants, as well as the protrusion of columns of ice from certain soils, we are now prepared to offer something in explanation of them, and to attempt to rise from the mass of details to the *causes* which have given birth to these remarkable appearances. A careful examination and collation of the two series of facts above recorded develope so many strong points of analogy, that it is almost impossible to resist the conviction, that both of the phænomena must be referred to the *same cause*. If we admit an identity of cause in the two cases, it is obvious that it must be purely *physical*; since that which relates to the production of the phænomenon on certain kinds of earth is necessarily physical. In the remarks which follow, therefore, I shall treat the question as one of physics, and shall apply them more particularly to the phænomenon exhibited by the soil: their application to the case of vegetables will be easy and obvious.

1. It is very clear that we cannot look to the store of vapour in the general atmosphere for the origin of the icy columns. For not only are the appearances above described at

variance with the idea of the phænomenon being a modification of hoar-frost, but the amount of water congealed at the surface during a single night is vastly too great to have come from this source. Moreover, the phænomenon occurs very conspicuously during our most violent and dry northwest winds; circumstances under which it would be impossible for any condensation of atmospheric vapour to take place. It is well known to meteorologists, that a rapid agitation of the lower strata of the atmosphere totally subverts the condition which is most essential to the deposition of dew, namely, that the surface must be *colder* than the superincumbent air.

2. It cannot be occasioned by the cold contracting a superficial stratum of earth, and thus forcing up the moisture which freezes at the surface, because this cause is utterly inadequate to furnish the large supply of fluid which is required for the production of columns of ice from three to five inches in length. The fact that isolated clods lying in moist situations frequently exhibit a protruded investment of icy columns quite equal in volume to the mass of earth from which they issued, is obviously and palpably at variance with this idea. The phænomenon observed on the stems of plants is likewise manifestly inconsistent with this notion.

3. It cannot be owing to the exhalation of aqueous vapour from the comparatively warm earth beneath through spiracles, undergoing condensation and congelation at the surface, and thus protruding the columns; for the amount of evaporation from such a surface, when the temperature of the air is at 12 or 14 degrees of Fahrenheit's scale, is hopelessly inadequate to furnish the necessary amount of water. Frequently during a single night a sufficient quantity of moisture is elevated in the form of icy columns to maintain the surface in a very wet condition, even after several days' exposure to the sun.

4. Neither can the protrusion of the columns of ice be ascribed to the mere expansion of water during the act of freezing in the capillary tubes in the clay; for this supposition is opposed to the well-established fact, *that they are not connected with any formation of ice below.* Besides, if we assume the specific gravity of ice to be ·92 as compared with water at 32° F., it follows that the amount of expansion which it undergoes during the process of congelation is about 87 parts in 1000 by volume. Granting the rigidity of the capillary tubes to be such as to admit of no transverse increment, and that the *whole* amount of cubic expansion is thereby manifested in the longitudinal extension of the column, it appears, from a simple calculation, that to protrude three inches of ice the frozen column must penetrate *about thirty-four inches* be-

low the surface of the soil. We have already seen that the ice *does not* extend below the surface when the phænomenon is well developed; and it is well known that the degree of cold necessary for freezing water is never observed in this latitude at a greater depth than one or two inches.

5. In seeking for a cause of the elevation of the fluid, the first suggestion which presented itself to my mind was the well-known and remarkable expansion which water undergoes before congelation commences. In this we have a *vera causa*, of sufficient universality and acting in the *right direction*, to account for the phænomenon, and yet perfectly consistent with an important *invariable concomitant* circumstance, namely, the *unfrozen* condition of the clay. But a little reflection very soon convinced me that it must play a subordinate part in the production of the phænomenon. A simple calculation is sufficient to place the inadequacy of this cause in a striking point of view. According to the recent and very satisfactory experiments of Joule and Playfair, the maximum density of water is at 39°·1 of Fahrenheit's scale (Phil. Mag. 3rd Series, vol. xxx. p. 41 *et seq*. 1847). The very elaborate series of experiments of Prof. Hällström show that the *mean expansion* in volume between the point of maximum density and the freezing-point (32° F.) is about 412 parts in 10,000,000 (Thomson On Heat, &c., p. 28. Lond. 1830). Hence it is obvious, that if, by the unyielding character of the capillary tubes, the whole of the increase of volume contributed to the elongation of the column, the length of the column of water requisite for furnishing 3 inches of ice through the operation of this cause would be about 72,815 inches, or nearly 6068 feet*. This reasoning is based upon the assumption, that the temperature of the water at the orifice of the tube is at 32°, while that at the other extremity of the column (viz. 6068 feet below the surface) is at 39°·1 F.; the only supposition consistent with the absence of ice beneath. As the effects of cold penetrate but a comparatively short distance below the surface of the earth, the insufficiency of this cause is too apparent to deserve further notice.

Having thus shown the inadequacy of several presumed causes to produce the remarkable phænomena under consideration, it is of course expected that we should offer some ex-

* According to an extensive series of experiments made by M. Despretz, the *mean* expansion of water between the points of maximum density and freezing, is 482·6 parts in 10,000,000. (Vide Pouillet's *Eléments de Physique Expérimentale et de Météorologie*, 5th edition, vol. i. p. 293. Paris, 1847.) This makes the required length of column equal to about 62,163 inches, or 5180 feet.

planation of them. Before doing so, it may be well to premise, that whatever may be thought to be the proximate cause of these phænomena, all the rules of philosophizing require us to look to the *earth* for the supply of fluid, and to the influence of *cold* for the elevating force. We have seen that the effect is *invariably* connected with cold, that it *increases* or *diminishes* with the intensity of the frigorific influence, and that it is *proportional* to the depression of temperature in all cases of *unimpeded* action. The whole difficulty lies, therefore, in ascertaining the *modus operandi* of this cause.

After considerable reflection, we venture to offer the following as the most probable explanation of the phænomenon. Let us suppose a portion of tolerably compact porous and warm earth, saturated with moisture, to be exposed to the influence of a cold-producing cause. The soil being an indifferent conductor of heat, only a very superficial stratum would be reduced to the freezing-point. As the resistance to lateral expansion is less at the surface than it is at a sensible depth below, the effect of the first freezing would be to render the apices of the capillary tubes or pores conical or pyramidal. The sudden congelation of the water filling the conical capillaries in the superior stratum would produce a rapid and forcible expansion, which, being resisted by the unyielding walls of the cone, would not only protrude, but *project* or *detach and throw out* the thread-like columns of ice in the direction of *least resistance*, or perpendicular to the surface. This would leave the summits of the tubes partially *empty*,—a condition essential to the development of capillary force. Under these circumstances, capillary attraction would draw up warm water from beneath, which, undergoing congelation, would in like manner elevate the column of ice still higher; and thus the process would go on as long as the cold continued to operate on unobstructed capillaries supplied with sufficient water from below. It will be remarked, that this explanation makes the whole process of protrusion to take place in a stratum of earth of almost inappreciable thickness. It also presumes that the protruding force acts *paroxysmally*. Does not the *wavy striated structure* of the icy columns clearly indicate that the freezing process is *intermittent*? It is obvious that the *unfrozen state* of the soil is maintained through the operation of two causes; to wit, the unceasing supply of warm water from below, and the large amount of *latent heat* evolved during the continued process of congelation. These two causes appear to be fully adequate to explain this remarkable fact.

The foregoing view explains why the phænomenon does not take place on hard-beaten earth and on very loose soils; for,

in the one case, the compactness of the superficial stratum not only diminishes the porosity, but renders the resistance to lateral expansion greater at the surface than it is below, and consequently interferes with the protrusion of the column of ice in the right direction; while in the other case, the openness of the soil prevents the formation of tubes possessing unyielding sides, a condition which is equally essential to the process. When the intensity of the cold is sufficiently great to freeze the soil, the process is arrested, because the capillary tubes are closed, and a resistance opposed to further protrusion. The porous appearance presented by the subjacent clay when the icy columns are removed is doubtless referable to the enlargement of the orifices of the minute capillaries, caused by the sudden expansion of the successive portions of fluid as they were frozen at the surface.

If the above is the true explanation of the phænomenon, we should expect, from *à priori* considerations, that in higher latitudes, where the cold is more intense and persistent, the conditions of its manifestation would exist only during the early part of winter, before the ground became deeply and permanently frozen; or else only in certain favourable situations,—as in the neighbourhood of warm springs, and perhaps along the margins of unfrozen streams,—where local causes preserved the soil in a proper condition. Are not facts in accordance with this view?

The foregoing explanation appears to afford a satisfactory interpretation of a very remarkable experiment recorded by Sir John Leslie, which is so nearly the counterpart of what takes place in nature, that we cannot forbear citing it on this occasion. He says, in treating of artificial congelation, "When very feeble powers of refrigeration are employed, a most singular and beautiful appearance is, in course of time, slowly produced. If a pan of porous earthenware, from four to six inches wide, be filled to the utmost with common water till it rise above the lips, and planted above a dish of ten or twelve inches diameter, containing a body of sulphuric acid, and then a broad round receiver placed over it; on reducing the included air to some limit between the twentieth and the fifth part of its usual density, according to the coldness of the apartment, the liquid mass will in the space of an hour or two become entwined with icy shoots, which gradually enlarge and acquire more solidity, but always leave the fabric loose and unfrozen below. The icy crust which covers the rim, now receiving continual accessions from beneath, rises perpendicularly by insensible degrees. From each point on the rough surface of the vessel, filaments of ice, like bundles of spun-

glass, are protruded, fed by the humidity conveyed through its substance, and forming in their aggregation a fine silvery surface, analogous to that of fibrous gypsum or satin spar." (Supplement to *Encyclopædia Britannica*, vol. iii. art. *Cold*, p. 258.) The same elevating cause must have been in operation during the progress of this experiment, which produces the protrusion of icy columns from the earth *.

The phænomenon manifested on certain plants is every way analogous to that relating to the protrusion of ice from certain kinds of soil, and admits of the same explanation. The porous pith furnishes a constant supply of warm water from the earth, while the wedge-shaped *medullary rays* secure the mechanical conditions necessary for the development of a projectile force in the proper direction. In proof of this it may be remarked, that the medullary rays are very conspicuous in the *Pluchea*; and when the stalk is split by the freezing of the water within, the fissure is observed to follow their course. The development of the phænomenon is arrested when the pith becomes frozen, for the obvious reason that the consequent splitting of the stem destroys the arrangement of resisting tubes. For a like reason it is exhibited lower down the stalk when it becomes rifted; for the conditions essential to its production are there found. When the cold-producing cause is not too intense, the stalk is not frozen, for the same reason that the ground remains unfrozen under similar circumstances. The reason why the phænomenon is manifested only in certain kinds of plants, probably arises from several peculiarities in their physical condition. They must be porous to furnish an abundant supply of fluid; they must be herbaceous and annual to secure *medullary rays* of sufficient size and openness; and it is probable that all vital action must have ceased, in order that the fluid

* Since writing the above, my attention has been called to analogous phænomena developed during the crystallization of certain salts.

If the smaller portions of the soft and spongy roots of our common cypress (*Cupressus disticha*, Mich.) be thoroughly soaked in a solution of nitrate of potash, and exposed to the drying influence of the air at ordinary temperatures, the whole surface will in process of time be covered with a most delicate investment of hair-like crystalline fibres. They are always observed to emanate at right angles to the convex surface of the root in the form of radial prolongations, and often extend out from the wood to a distance equal to, or exceeding its semi-diameter. When brushed off by the finger, a fresh crop speedily appears. A similar exudation of crystalline filaments of sulphate of zinc is frequently observed on the surface of the porous earthenware cups used in Grove's battery, when they are not carefully washed. It is obvious that these phænomena are identical in their origin with the protrusion of the columns of ice from the earth, excepting that in the *latter case* the influence of *cold* is essential to the process of crystallization.

which is elevated from the soil may be unmixed with the proper juices of the plants, a mixture which would interfere with congelation.

We conclude these observations with a few remarks on the teleological bearing of the phænomenon which we have been considering. The laws of the effect of temperature on water are so remarkable in their adaptation to the beneficial course of things at the earth's surface, that they have never failed to impress the student of Nature with the most profound admiration of the wisdom and goodness of the Great Designer. Among these, the infinite importance of the *latency of heat* in the œconomy of nature is one of the most striking. In the phænomenon which we have had under consideration in relation to the protrusion of icy columns from the earth, we recognize an *extension* of this law, the importance of which it is scarcely possible for us to over-estimate. By an admirable combination of the laws of expansion and capillary attraction, a vast amount of water is brought to the surface of the soil, and there disengages its *latent heat* in the act of congelation, thereby softening the rigours of winter, and preserving the roots or bulbs beneath the surface of the ground from the destructive effects of cold. Even on those portions of the soil where the phænomenon does not manifest itself in the protrusion of columns of ice, it is extremely probable that the same law operates to a more limited extent. This seems to be proved by a fact, which must have come under the observation of every one; namely, that the amount of moisture found at the surface of the ground *after* a thaw is vastly greater than was present *before* congelation took place. This is the case, under circumstances which are incompatible with the idea of the deposition of dew; the water must therefore have been elevated from the depths of the earth. The philosopher who loves to dwell on causes and effects, and to trace the deep processes of thought by which the great purposes of nature have been revealed, both in the heavens above and in the physical condition of the earth on which he treads, will be gratified to discover in every portion of the universe those prospective arrangements, compensations, minute adaptations, and comprehensive inter-dependencies, which characterize the works of an omniscient Architect.

Athens, Georgia, Feb. 25, 1850.

XLII. *On the Theory of the Tides.* *By the* Rev. Brice Bronwin.

[Continued from p. 197.]

IF, in order to find the terms depending on $\sin(\varphi-\epsilon_1)$ and $\cos(\varphi-\epsilon_1)$, we combine u_2 and v_2 with u_1 and v_1, there will result terms with constant coefficients containing the factor a_1a_2. And though they might in many places be larger than those before found containing $a^2{}_1$, they would be very much less than those of the first order; and, moreover, we may suppose them included in those terms of the first order which have constant coefficients. Besides, if we take account of terms thus formed, we ought also to take account of the terms depending on $\sin 3(\varphi-\epsilon_3)$ and $\cos 3(\varphi-\epsilon_3)$, which must be at least as large.

Combining u_1 and v_1 with u_0 and v_0, we find from (3.) the terms

$$\delta\theta(\sin^2\theta-\cos^2\theta)2n^2\mathrm{D}\sigma\cos(\varphi-\epsilon_1)+\delta\varpi\{(\sin^2\theta-\cos^2\theta)2n^2\mathrm{C}\sigma-2n^2\sin\theta\cos\theta\mathrm{D}\sigma\}\sin(\varphi-\epsilon_1)-\delta\theta(\mathrm{C}+2\sin\theta\cos\theta\mathrm{D})n^2\frac{d\sigma}{d\theta}\cos(\varphi-\epsilon_1)-\delta\theta(\sin^2\theta\mathrm{D}+2\sin\theta\cos\theta\mathrm{C})n^2\frac{d\tau}{d\theta}\sin(\varphi-\epsilon_1).$$

This, to abridge, may be written

$$\left.\begin{array}{l}\delta\theta\left(\mathrm{G}'\sigma-\mathrm{H}'\dfrac{d\sigma}{d\theta}\right)\cos(\varphi-\epsilon_1)+\delta\varpi\mathrm{K}'\sigma\sin(\varphi-\epsilon_1)\\ \qquad-\delta\theta\mathrm{L}'\dfrac{d\tau}{d\theta}\sin(\varphi-\epsilon_1).\end{array}\right\}\cdot \quad (15.)$$

Observing that $\frac{d\mathrm{C}}{d\theta}=0$, the equation of continuity gives the terms

$$\left\{\mathrm{D}\left(\frac{d\sigma}{d\theta}+\frac{\sigma\cos\theta}{\sin\theta}\right)+\mathrm{C}\frac{d\sigma}{d\theta}\frac{\cos\theta}{\sin\theta}\right\}\cos(\varphi-\epsilon_1)+\mathrm{C}\frac{d\tau}{d\theta}\sin(\varphi-\epsilon_1)-\frac{1}{2}\sigma^2,$$

which may be written

$$-\frac{1}{2}\sigma^2+\left(\mathrm{M}'\sigma+\mathrm{N}'\frac{d\sigma}{d\theta}\right)\cos(\varphi-\epsilon_1)+\mathrm{C}\frac{d\tau}{d\theta}\sin(\varphi-\epsilon_1). \quad (16.)$$

As before, we neglect the small quantity $-\frac{1}{2}\sigma^2$, since it is not of the form of the terms we are seeking; and we see that (15.) and (16.) are exactly similar to (5.) and (7.), and therefore by exactly the same steps as those employed upon them

we shall find equations similar to (8.), (9.) and (10.), and consequently that Δu and Δv are here of the form

$$P' \sin(\phi - \epsilon_1) + Q' \cos(\phi - \epsilon_1),$$

P' and Q' having σ, $\frac{d\sigma}{d\theta}$, or $\frac{d\tau}{d\theta}$ as factors in every term, and also na_1. P' and Q' are therefore of the order $na_1\sigma$, $na_1\tau$.

As the terms previously found are of the same form as Δu and Δv, the sum of the results will be of the same form and of the same order of magnitude.

We must now change $D_1 \cos(\phi - \epsilon_1)$ of vol. xxxv. p. 265 into

$$D_1 \cos(\phi - \epsilon_1) + D_0 \sin(\phi - \epsilon_1).$$

Then instead of the third and fourth of (12.), we shall have

$$\left.\begin{aligned} F_1 \cos\beta_1 &= D_1 \cos\epsilon_1 + E_1\rho^3 \sin v \cos v - D_0 \sin\epsilon_1 \\ F_1 \sin\beta_1 &= D_1 \sin\epsilon_1 + D_0 \cos\epsilon_1. \end{aligned}\right\} \quad (17.)$$

Whence we have

$$F_1^2 = D_1^2 + 2D_1E_1\rho^3 \cos\epsilon_1 \sin v \cos v + E_1^2\rho^6 \sin^2 v \cos^2 v,$$

neglecting

$$-2D_0E_1\rho^3 \sin\epsilon_1 \sin v \cos v + D_0^2.$$

Suppose

$$F_1 = D_1 + E_1\rho^3 \cos\epsilon_1 \sin v \cos v, \quad . \quad . \quad . \quad . \quad (18.)$$

as in (15.), page 266, except that D_1 has changed its value by the addition of some small variable quantities.

We cannot, however, affirm (18.) to be a near approximation unless we are sure of D_1 being considerably larger than E_1, nor can we reduce the following,

$$\sin\beta_1 = \frac{D_1}{F_1} \sin\epsilon_1 + \frac{D_0}{F_1} \cos\epsilon_1,$$

but upon the same supposition.

We must now change D_1 into $D_1 + m' \sin^2 v$, and put for $\cos\epsilon_1$ its value from (17.), page 266 (paper referred to), or

$$\cos\epsilon_1 = \cos k_1 - \frac{1}{4} \sin^2 o \sin k_1 \sin 2z + 2e \sin k_1 \sin(z - \pi).$$

Thus, neglecting some very small quantities, we shall find

$$F_1 = D_1 + E_1 \cos k_1 \sin v \cos v + m' \sin^2 v + 3eE_1 \cos k_1 \sin v \cos v$$
$$\cos(z - \pi) + 2eE_1 \sin k_1 \sin v \cos v \sin(z - \pi), \quad . \quad . \quad (19.)$$

the accuracy of which will depend upon the supposition of D_1 being considerably larger than E_1. On this supposition

$$\sin\beta_1 = \sin\epsilon_1 - \frac{E_1}{D_1}\rho^3 \sin\epsilon_1 \cos\epsilon_1 \sin v \cos v + \frac{D_0}{F_1} \cos\epsilon_1,$$

and

$$\beta_1 = \mathfrak{b}_1 - \frac{E_1}{D_1}\rho^3 \sin \mathfrak{b}_1 \sin v \cos v + \frac{D_0}{F_1} \text{ nearly.}$$

Change D_1 into $D_1 + m' \sin^2 v$, and make

$$\frac{D_0}{F_1} = \frac{p' + q' \sin^2 v}{D_1}$$

in conformity with the similar assumption made in the last paper, and on the same grounds; then

$$\beta_1 = \mathfrak{b}_1 - \frac{E_1}{D_1}\rho^3 \sin \mathfrak{b}_1 \sin v \cos v + \frac{p'}{D_1} + \frac{q'}{D_1} \sin^2 v$$

$$= \mathfrak{b}_1 - \frac{E_1}{D_1} \sin \mathfrak{b}_1 \sin v \cos v + \frac{p'}{D_1} + \frac{q'}{D_1} \sin^2 v + \frac{3eE_1}{D_1}$$

$$\sin \mathfrak{b}_1 \sin v \cos v \cos (z - \pi).$$

Putting for $\mathfrak{b}_1$ its value from (17.), page 268 (paper referred to), and reducing the result, we find

$$\beta_1 = k_1 + p' + \frac{1}{4} \sin^2 o \sin 2z - \frac{E_1}{D_1} \sin k_1 \sin v \cos v - 2e \sin(z - \pi)$$

$$+ q' \sin^2 v + \frac{3eE_1}{D_1} \sin k_1 \sin v \cos v \cos (z - \pi). \quad . \quad . \quad (20.)$$

Here D_1 is constant, as in the terms of the first order, and $\frac{p'}{D_1}, \frac{q'}{D_1}$ have been changed into p' and q'. The last might perhaps be put under a somewhat more simple form, but we have not much ground for attempting a general reduction of the quantities F_1 and β_1.

If for any particular place we can make a near estimate of the comparative values of D_1 and E_1, we may perhaps be able to put the expressions of these quantities under a better form. It is evident from (17.) that they may vary very greatly from one place to another. If the diurnal equation be very small or insensible on our shores, it is possible that it may be very large in some other places. But this will chiefly depend upon the form of the shores and of the bottom of the sea.

It may happen in some places that the supposition on which we have obtained approximate values of F_2, β_2 may not be admissible. In that case we must have recourse to the equations (11.), and endeavour to deduce the values of these quantities from them otherwise. The values of these quantities in this theory have been deduced from a supposition which we know to be true on our own shores.

It may not be amiss to ascertain whether the disturbing

force of the water be able to produce any sensible effect. For this purpose let

$$D=\sqrt{r^2-2rr'\cos\chi+r'^2)}=\sqrt{2-2\cos\chi}=2\sin\frac{\chi}{2},\ r=r'=1,$$

r and r' being the radii of the disturbed and disturbing particles, and χ the angle contained between them, and neglecting the product of the ellipticity by the height of the tide. Then D is the distance of the two particles, and for this force

$$V=\int\frac{dM}{D},$$

where

$$dM=y'\sin\theta' d\theta' d\varpi',$$

y' being the height of the tide where the disturbing particle is situated, θ' the colatitude of this particle, and $\varpi'-\varpi$ its longitude from the meridian of the disturbed particle. Hence

$$V=\iint\frac{y'\sin\theta' d\theta' d\varpi'}{2\sin\frac{\chi}{2}},$$

the integral being taken from $\theta'=o$ to $\theta'=\pi$, and from $\varpi'=o$ to $\varpi'=2\pi$. Let

$$dP'=\frac{\sin\theta' d\theta' d\varpi'}{2\sin\frac{\chi}{2}},$$

then

$$V=\int y'dP';$$

also

$$\cos\chi=\cos\theta\cos\theta'+\sin\theta\sin\theta'\cos(\varpi'-\varpi).$$

Where the sea rises above the equilibrium height, y' is positive; where it sinks below it, y' is negative; and where it is at that height, or where there is no sea, it is nothing.

Let

$$y=h+h_1\sin^2 v+h_2\sin v\cos v+h_3\sin 2z+\&c.$$

be the part of y which is constant, or which contains only equations of long periods; then

$$y'=h'+h'_1\sin^2 v+\&c.,$$

and for this part of y',

$$V=\int h'dP'+\sin^2 v\int h'_1 dP'+\ \&c.$$

The accent denotes that θ is changed into θ', and the arbitrary constants in y into those of y' at the place of the disturbing particle, these arbitraries varying from place to place, and

the integration being relative to their variation equally with the variation of θ'. Now, making

$$\frac{1}{g}\int h' d\mathrm{P}' = k, \quad \frac{1}{g}\int h'_1 d\mathrm{P}' = k_1, \text{ \&c.},$$

we have

$$y = \frac{\mathrm{V}}{g} = k + k_1 \sin^2 v + \text{ \&c.}$$

for the height of the tide resulting from the disturbing force of the sea, which therefore introduces no new terms, but only makes an alteration in the constant coefficients.

Now let

$$y = \Sigma \mathrm{F}_i \cos i(\phi - \beta_i)$$

be the sum of the equations of short periods. Make

$$\beta_i = k + \lambda,$$

k being constant, and λ variable and very small. Then

$$y = \Sigma \mathrm{F}_i \cos i(\phi - k) + \Sigma \mathrm{F}_i \lambda \sin i(\phi - k)$$

$$= \Sigma(\cos ik - \sin ik\lambda)\mathrm{F}_i \cos i\phi + \Sigma(\sin ik + \cos ik\lambda)\mathrm{F}_i \sin i\phi.$$

But

$$\mathrm{F}_i = f + x,$$

f being constant, and x variable and small; we may therefore write

$$y = \Sigma(\mathrm{A} + \mathrm{B}\mu) \cos i\phi + \Sigma(\mathrm{C} + \mathrm{D}\nu) \sin i\phi,$$

μ and ν being variable or functions of t. Consequently

$$y' = \Sigma(\mathrm{A}' + \mathrm{B}'\mu') \cos i\phi' + \Sigma(\mathrm{C}' + \mathrm{D}'\nu') \sin i\phi',$$

where

$$\phi' = nt + \varpi' - \psi = nt + \varpi - \psi + (\varpi' - \varpi) = \phi + (\varpi' - \varpi).$$

With this value of ϕ' we have

$$y' = \Sigma\{(\mathrm{A}' + \mathrm{B}'\mu') \cos i(\varpi' - \varpi) + (\mathrm{C}' + \mathrm{D}'\nu') \sin i(\varpi' - \varpi)\} \cos i\phi$$

$$+ \Sigma\{-(\mathrm{A}' + \mathrm{B}'\mu') \sin i(\varpi' - \varpi) + (\mathrm{C}' + \mathrm{D}'\nu') \cos i(\varpi' - \varpi) \text{ s in} i.$$

By changing the values of A', B', &c., and also those of μ' and ν', this result may still be written,

$$y' = \Sigma(\mathrm{A}' + \mathrm{B}'\mu') \cos i\phi + \Sigma(\mathrm{C}' + \mathrm{D}'\nu') \sin i\phi,$$

which gives

$$\mathrm{V} = \Sigma\left(\int \mathrm{A}' d\mathrm{P}' + \int \mathrm{B}'\mu' d\mathrm{P}'\right) \cos i\phi + \Sigma\left(\int \mathrm{C}' d\mathrm{P}' + \int \mathrm{D}'\nu' d\mathrm{P}'\right)$$
$$\sin i\phi.$$

To perform the integrations we must put

$$\mu' = m + m_1 \sin^2 v + \text{ \&c.},$$

$$\nu' = n + n_1 \sin^2 v + \text{ \&c.}$$

Then making

$$\frac{1}{g}\int A' dP' = G, \quad \frac{1}{g}\int C' dP' = K, \text{ \&c.,}$$

we shall have

$$y = \frac{V}{g} = \Sigma(G + H\mu_1)\cos i\varphi + \Sigma(K + L\nu_1)\sin i\varphi$$

for the effect of the disturbing force of the sea arising from equations of short periods. This is precisely similar to the value of y given above, resulting from the action of the planet. This force, therefore, if sensible, makes no alteration in the general form of y, but only in the value of the numerical coefficients. This investigation may also include the irregularity of the action of gravity arising from high mountains and shores, which may in some places produce as much effect as the disturbing force of the water itself. But the effect of these causes is generally insensible; it is therefore unnecessary to dwell upon it, and the more so as we are not able to compute its exact numerical amount.

If the additional terms in the value of y which have been found in these two papers be very small in some places, as they no doubt will be, they may yet be considerable in others, and it may be necessary, therefore, to take account of them in order that theory and observation may everywhere accord.

Gunthwaite Hall, near Barnsley, Yorkshire,
March 7, 1850.

XLIII. *On the Nitroprussides, a New Class of Salts.* *By* Dr. Lyon Playfair, *F.R.S., F.C.S.*

[Concluded from p. 283.]

Section IV.—*Action of Caustic Alkalies on the Nitroprussides.*

25. When a dissolved caustic alkali, such as potash or soda, is added to a solution of a nitroprusside, the dark red colour of the solution changes to an orange-yellow. If both solutions have been moderately dilute, no oxide of iron is precipitated, nor is ammonia evolved. The addition of alcohol to the orange-yellow liquid causes the precipitation of an aqueous solution of a new salt. This salt may be procured in a solid state as follows. Nitroprusside of potassium is dissolved in water and double its volume of alcohol is added. Caustic potash is now added to this solution, and a yellow curdy precipitate is obtained. This precipitate is washed with alcohol to free it from an excess of either of the reagents, but it is almost impossible to remove the last traces. The salt is now

pressed between folds of bibulous paper and dried *in vacuo* over sulphuric acid. It may be called nitroprusside of potassium and potash.

This salt is of a bright yellow colour and of crystalline appearance. It is very sparingly soluble in alcohol, but very soluble in water, to which it gives a strong alkaline reaction. It precipitates salts of lead of a fine yellow colour like the chromate of lead. Salts of iron are precipitated of a yellowish-brown, and salts of copper of a brown colour. On the addition of an acid, the excess of potash is removed and nitroprusside of potassium remains in solution; the salt therefore is a compound of a nitroprusside with potash. It will not crystallize *in vacuo*, its solution decomposing with the deposition of an oxide of iron, and with the escape of a gas which communicates a pink colour to the sulphuric acid used for the evaporation in the air-pump. The salt heated in a tube evolves nitric oxide and ammonia, and leaves a black residue which yields to water an alkaline solution of a nitroprusside. When its solution in water is boiled, complete decomposition takes place, a ferrocyanide, oxide of iron, nitrite and oxalate of potash being produced.

It is almost impossible to obtain it free from uncombined nitroprusside, which is observed to remain in solution when a salt of lead is added to it. If potash in excess be used, it is equally difficult to remove the excess by washing. The analyses therefore give only approximative results; they were made in the usual way by decomposing the salt with fuming sulphuric acid.

I. 17·350 grs. gave 3·440 grs. $Fe^2 O^3$ and 14·32 KO, SO^3.

II. 37·870 grs. gave 7·345 grs. $Fe^2 O^3$ and 30·53 KO, SO^3.

The combustions were made with chromate of lead.

I. 14·075 grs. gave 7·765 grs. CO^2 and 1·015 gr. HO.

II. 13·71 grs. gave 7·490 grs. CO^2 and 0·985 gr. HO.

The samples of salt analysed were made at different times.

	I.	II.	Mean.			Calculated.
Iron . .	13·87	13·57	13·72	5	140	14·38
Potassium	37·00	36·14	36·57	9	351	36·07
Carbon .	15·04	14·89	14·96	24	144	14·79
Hydrogen	0·80	0·79	0·79	8	8	0·82
Nitrogen } Oxygen }	33·29	34·61	33·96	15 15	210 120	33·94
	100·00	100·00	100·00		973	100·00

Hence this salt differs from nitroprusside of potassium by containing 4 atoms of potash attached. Its formula is therefore $Fe^5 Cy^{12} 3NO, K^5 + 4KO + 8HO$. There is little doubt

that it might, when quite free from nitroprusside, contain an additional equivalent of potash.

It has been stated that a solution of this salt is decomposed on boiling. Oxide of iron falls down, nitrogen escapes, and the solution is now found to contain ferrocyanide of potassium, nitrite of potash and traces of oxalate of potash.

26. The products of transformation were determined (1) by precipitating the ferrocyanide by alcohol; (2) by adding nitrate of lime to precipitate the oxalate*, which was always accompanied by a minute quantity of a pink compound containing cyanogen and iron; (3) by examining the liquid which remained, and was found to evolve nitric oxide on the addition of an acid. It gave a precipitate with nitrate of silver, which, though sparingly soluble in cold water, dissolved in hot water and crystallized on cooling; 13·25 grs. of the crystalline salt thus obtained, treated with hydrochloric acid, gave 12·33 grs. chloride of silver, or 70·03 per cent. Nitrite of silver (AgO, NO^3) contains 70·12 per cent.

In examining the relative quantities of these products of transformation, recourse was first had to the yellow salt itself. But as this generally contained a little nitroprusside, and as the products of decomposition varied with the period of ebullition, on account of the slower action from the insufficient quantity of alkali, it was found more accurate to examine the transformations by acting upon a solution of nitroprusside with an excess of alkali. Without therefore giving the details of the experiments on the yellow salt itself, some of the general results may be stated; from these it will be seen that the quantities of oxide of iron and of prusside produced vary according to the conditions of the experiment, principally according to the longer or shorter period of ebullition. 100 parts of the yellow salt gave, on boiling its aqueous solution,—

	I.	II.	III.	IV.	V.
Peroxide of iron . . .	3·0	3·58	3·0	3·56	2·71
Ferrocyanide of potassium	60·86	60·59	59·48	68·83	64·50

In all these cases there was more or less nitroprusside of potassium undecomposed. The amount of oxalate of potash found in solution varied from 0·97 to 1·5 per cent.

The transformation was now examined in the following

* To prove that this was an oxalate, a portion was precipitated by nitrate of lead from the solution after precipitation by alcohol. The precipitate was of a pink colour, and was now decomposed by sulphuretted hydrogen, neutralized by pure carbonate of soda, and again precipitated as a lead salt, which was now quite white. Calcined with nitrate of ammonia, 1·660 gr. gave 1·250 gr. oxide of lead, or 75·3 per cent. Oxalate of lead contains 75·5 per cent.

manner. A weighed quantity of a nitroprusside was dissolved in water and boiled, caustic potash or caustic soda (according as the nitroprusside was a salt of potassium or sodium) being added to the boiling solution, until a drop taken out gave, after being neutralized, no purple colour with a sulphide. The precipitated oxide of iron was now collected and weighed. The filtrate was precipitated by alcohol, and the prusside collected and determined on a weighed filter. The filtrate was now neutralized with acetic acid, and chloride of calcium added, but the oxalate of lime was generally not in sufficient quantity to collect and weigh, mere traces being obtained. It was now attempted to estimate the amount of nitrate by the process described by Nesbit for analysing nitrates*, that is, by converting its nitrogen into ammonia by zinc and muriatic acid, the hydrogen being slowly evolved. The ammonia thus formed was separated by distillation with caustic soda, collected in muriatic acid and determined as chloride of platinum and ammonia. This process did not however give constant results in my hands, probably from the difficulty of preventing the escape of nitric oxide on adding an acid to the nitrite. The nitrite was therefore determined by loss. In one case only did I, by the above process, obtain a result approaching the quantity of nitrite in solution.

17·24 grs. of nitroprusside of sodium were dissolved in water, the solution was boiled and caustic soda added, keeping the solution distinctly alkaline after ebullition had continued for some time. It yielded 0·92 gr. peroxide of iron, and 14·85 grs. ferrocyanide of sodium; the residual liquid, treated according to Nesbit's plan, only gave 2·57 grs. platinum salt.

Iron precipitated . . .	3·73 per cent.
Iron in prusside . . .	15·08 per cent.
	18·81

Hence all the iron, except about 0·5 per cent., is found in the oxide of iron and in the prusside; the remainder is probably in the minute quantity of pink salt alluded to above. The carbon contained in the prusside amounts to 20·3; so that the total quantity of cyanogen has gone down in that form, the carbon in the nitroprusside being 20·0 per cent.

It will be seen that the iron precipitated as peroxide of iron is one-fourth that retained in the ferrocyanide. The following equation expresses the transformation:—

$$2(Fe^5 Cy^{12}\, 3NO, Na^5) + 9NaO = 4(Fe^2 Cy^6, Na^4) + 3NaO, NO^3 + Fe^2 O^3 + 3N.$$

* Memoirs of Chemical Society.

Or expressed in another way,—

4 equivs. ferrocyanide of sodium	Fe^8	Cy^{24}	Na^{16}		
3 equivs. nitrite of soda . . .			Na^3	N^3	O^{12}
1 equiv. peroxide of iron . .	Fe^2				O^3
3. equivs. nitrogen				N^3	
2 equivs. nitroprusside + 9 of soda =	Fe^{10}	Cy^{24}	Na^{19}	N^6	O^{15}

The first change is obviously to form ferrocyanide of sodium, 6 equivs. of oxygen passing over to the nitrous oxide; this, with the oxygen in the latter, would make 4 equivs. nitrous acid; but the 2 equivs. of iron liberated require 3 of oxygen to form peroxide, which it receives at the expense of the nitrous acid, leaving therefore 3 equivs. of that acid to unite with soda, the remaining 3 equivs. of nitrogen escaping as a gas. During the ebullition no ammonia can be detected, either by smell or by turmeric paper.

Section V.—*Action of an Alkaline Sulphide on a Nitroprusside.*

27. It has been repeatedly mentioned, that when solutions of nitroprusside of potassium or sodium and of the corresponding sulphides are mixed together, the most magnificent purple colour is produced. This colour however is very transitory and cannot be preserved in an aqueous solution. The purple or blue compound may however be obtained in a solid state when alcoholic solutions of the two salts are employed. In order to obtain it in this state, nitroprusside of sodium is dissolved in the smallest possible quantity of water, and to this solution is added four or five times its bulk of alcohol. An alcoholic solution of neutral sulphide of sodium (the sulphide obtained by reducing the sulphate with hydrogen) is now added to the alcoholic solution of nitroprusside, the addition being stopped before the supernatant liquid gives a decidedly black reaction on lead paper. The mixed solutions acquire a magnificent purple blue colour. On stirring the mixture, an aqueous solution of the purple compound falls down in oily drops. After this has settled, the alcohol is decanted, and the blue solution is washed repeatedly and quickly with alcohol by decantation. It is now, as rapidly as posssible, put *in vacuo* over sulphuric acid, when it soon parts with its water and becomes solid. It usually dries to a dirty green powder, which is a mixture of the purple compound with the products of its decomposition. It may however, though this is rare, dry quite unchanged in its character, being still of a fine blue colour and dissolving entirely in water with all its magnificent

purple blue shade. It cannot then be dried in the water-bath, where it quickly decomposes and becomes green.

The following analysis was made on two portions which were dried in the air-pump, until they ceased to lose weight and had all their properties unchanged. They were oxidized by nitrate of ammonia; the residue was dissolved in nitric acid. The iron was precipitated as peroxide, the sulphur estimated as sulphate of barytes, and the soda as a sulphate.

I. 14·210 grs. gave 3·420 $Fe^2 O^3$, 5·710 BaO, SO^3, and 9·38 NaO, SO^3.

II. 8·99 grs. gave 3·88 BaO, SO^3, and 6·62 NaO, SO^3, the iron being accidentally lost.

The combustion was made by chromate of lead, peroxide of lead being used to arrest the sulphurous acid.

I. 6·20 grs. gave 3·855 grs. CO^2 and 0·440 gr. HO.

II. 10·565 grs. gave 6·810 grs. CO^2, and 0·675 gr. HO.

	I.	II.	Mean.
Iron . . .	16·84	16·84	16·84*
Sodium . .	21·37	23·84	22·60
Sulphur . .	5·51	5·92	5·71
Carbon . .	16·95	17·58	17·27
Hydrogen .	0·78	0·71	0·74
Nitrogen, Oxygen .	38·55	35·11	36·84
	100·00	100·00	100·00

In such a variable compound as this, close results can scarcely be looked for in two analyses. As an approximation, however, it will be seen that the iron is to the sodium as 5:8, and to the sulphur as 5 : 3.

The blue unchanged compound gives with protosulphate of iron a beautiful precipitate of the same purple blue colour as itself, but this is decomposed by washing. With salts of lead it gives a brownish-yellow precipitate, with salts of copper a brown precipitate, both these being obviously products of decomposition.

28. The purple blue compound dissolved in water speedily becomes red, and when in this state, a salt of lead throws down a pinkish red precipitate. This red solution however soon decomposes, a brownish precipitate falling, and the yel-

* It should be stated that in many analyses of this compound in its partially decomposed state, the most discordant results were obtained. The two analyses here adduced were made on the only specimens which appear to be unchanged; in all the other cases the compound had become green and therefore was decomposed, as it no longer dissolved in water with its characteristic purple tint.

low colour due to a prusside being seen in the solution. If the sulphide originally employed contained sulphuretted hydrogen, a soluble prussian blue is also found in the liquid. During these changes, ammonia, hydrocyanic acid, and a gas possessing the properties of nitrogen are given off. In fact, on mixing the solutions of sulphide and nitroprusside, it is difficult, even by keeping the solutions quite cold, to prevent the formation of a little ammonia and escape of nitrogen. The solution of the purple compound in water decomposes even under the air-pump, depositing the brown precipitate, and it does so immediately when it is boiled.

When the solution is filtered from the brown precipitate, the addition of alcohol separates ferrocyanide of sodium. The alcoholic filtrate strikes a blood-red colour with a persalt of iron, and with sulphuric acid evolves nitric oxide, which is immediately rendered sensible by a protosalt of iron, a nitrite being thus shown to be in solution. Ammonia cannot be detected in the solution, neither does it appear to any great extent when the transformation takes place in the cold, though it always does so when ebullition is used to hasten the transformation. It therefore appears to be the product of an after action.

The brown precipitate is first to be examined. It is found to consist of peroxide of iron and sulphur, the latter remaining when the former is dissolved out by an acid. It was analysed by oxidation with nitromuriatic acid. 7·21 grs. gave 16·90 grs. sulphate of barytes, equal to 2·33 grs. of sulphur, and 4·22 grs. peroxide of iron, the rest being water. Hence the proportion of sulphur to iron in equivalents is nearly as 4 : 3; the proportion for 2·33 sulphur would yield 3·0 iron, while 2·95 was found by the experiment.

It was now desirable to ascertain what proportion of iron was thrown down as ferrocyanide and how much remained in the brown precipitate. For this purpose a portion of a preparation, which had become green by standing in the air-pump, was first analysed in order to ascertain the relative proportion of its constituents, and it was then dissolved in water and boiled.

14·41 grs. gave 6·93 grs. sulphate of barytes, 17·68 grs. gave 3·55 grs. peroxide of iron and 10·00 grs. sulphate of soda. 6·025 grs. gave 3·31 grs. carbonic acid and 0·820 gr. water. Hence this changed purple compound, before complete transformation, contained in 100 parts,—

Iron	14·05
Sodium	18·33
Sulphur	6·59
Carbon	14·98
Hydrogen	1·51
Nitrogen, Oxygen	44·54
	100·00

11·31 grs. were now boiled in water, and 0·94 gr. of the brown precipitate was obtained by filtration, and 5·90 grs. of prusside of sodium were precipitated by alcohol. Hence of the total quantity of 1·58 gr. of iron present 1·08 gr. was found in the ferrocyanide, the remainder being in the brown precipitate. As the ferrocyanide of sodium is of constant composition, which the brown mixture is not, the iron in the latter is here estimated by loss and would amount to 0·50 gr. The proportion in equivalents is nearly, though not exactly, as 7 : 3, which would have made the iron in the brown precipitate 0·46 gr. instead of 0·50 gr.

Taking these proportions as leading to a general view of the transformation, it may be expressed by the following equation:—

$$2(Fe^5\,Cy^{12}\,3NO+8Na+3S)+2HO=7(Na^2,\,Fe\,Cy^3)$$
$$+(Na,\,Cy\,S^2)+(NaO,\,NO^3)+Fe^3\,O^4+S^4+2HCy+2N.$$

The only point in which this transformation does not agree with experiment, is in the supposed production of ferroso-ferric oxide, whereas, when the brown precipitate is washed with acid, only peroxide of iron unaccompanied by protoxide of iron passes through. It is therefore probable that the oxidation of this oxide may give rise to the small quantity of ammonia observed, the oxygen from decomposed water uniting with it, and the nascent hydrogen with nitrogen to form ammonia. Allowing this to be the explanation of the disagreement with experiment, the following scheme may render the above equation more immediately intelligible. Two equivalents of the blue compound with 2 equivs. of water, by boiling, are resolved into—

7 equivs. ferrocyanide of sodium.
1 equiv. sulphocyanide of sodium.
1 equiv. nitrite of soda.
1 equiv. oxide of iron ($FeO+Fe^2\,O^3$).
4 equivs. sulphur.
2 equivs. hydrocyanic acid.
2 equivs. nitrogen.

And probably the ferroso-ferric oxide is transformed at the expense of the oxygen of water into ferric oxide, the hydrogen forming ammonia with nitrogen,

$$6FeO + 3HO + N = 3Fe^2O^3 + NH^3.$$

29. In giving the above equation, the blue sulphur compound was supposed to consist of nitroprusside of sodium with 3 equivs. of sulphuret of sodium attached. The following calculation shows that this is an expression of the analysis:—

		Calculated.	Mean experiment.
5 Iron . . .	140	17·36	16·84
8 Sodium . .	186	23·07	22·60
24 Carbon . .	144	17·86	17·27
3 Sulphur .	48	5·95	5·71
6 Hydrogen .	6	0·74	0·74
15 Nitrogen .	210	35·02	36·84
9 Oxygen .	72		
	806	100·00	100·00

The approximation is sufficiently near when the difficulty of getting the substance in at all a stable state is considered. Two views might be taken of the constitution of this singular compound (1), that it is nitroprusside of sodium with 3 equivs. of sulphuret of sodium attached—

$$Fe^5 Cy^{12}\, 3NO,\, 5Na + 3NaS + 6HO;$$

but this would scarcely account for its extreme facility of decomposition; it may therefore be supposed that caustic soda is attached to the salt, as we have seen that it can be, in studying the action of alkalies on the nitroprussides, and that the sulphur has taken the place of the oxygen, thus:—

$$Fe^5 Cy^{12}\, 3NS,\, 5Na + 3NaO + 6HO.$$

Either of these formulæ would suit the analysis; in support of the latter may be adduced the fact observed by Gregory, that sulphuret of nitrogen in the presence of caustic alkalies acquires a deep transitory amethyst colour, which, on disappearing, evolved ammonia, a description exactly accordant with the present case.

Action of Sulphuretted Hydrogen on the Nitroprussides.

30. Sulphuretted hydrogen decomposes the soluble nitroprussides. The products of transformation are most conveniently obtained in the following way:—Nitroprusside of sodium is dissolved in the smallest possible quantity of cold water, and three or four times its volume of alcohol is added to the solution. Sulphuretted hydrogen is now passed through

this alcoholic solution. Sulphur, prussian blue, and ferrocyanide of sodium, are very gradually precipitated; the action, however, is very slow, and must be long continued. The alcoholic solution is now of a reddish olive-brown colour. When the sulphuretted hydrogen has ceased to act, this supernatant brownish liquid gives no coloration when mixed with an alkaline sulphide. If allowed to stand for a few hours, it deposits a little of the precipitates which it held in solution. After this the brown solution is found to contain neither ferrocyanide nor nitroprusside of sodium; a persalt of iron is slightly deepened in colour when mixed with it, showing the presence of a mere trace of a sulphocyanide. When this reddish-brown solution is evaporated in the water-bath, it deposits oxide of iron and sulphur, and becomes decomposed. Evaporated *in vacuo* over sulphuric acid, it deposits, when nearly dry, black crystalline needles, but these seem to be a product of decomposition, and are mixed with oxide of iron and other substances; attempts were therefore made to ascertain the composition of the original substance by precipitating its solution by metallic salts. Bichloride of mercury produces a brown precipitate, sulphate of copper a pinkish-brown, and nitrate of silver a black precipitate. But these were obviously products of decomposition, for during the precipitation nitric oxide is abundantly evolved. This is especially the case in the precipitate with silver. If that precipitate, after being washed, be now mixed with a small quantity of hydrochloric acid to take up the silver, sulphuretted hydrogen is evolved, protochloride of iron and abundance of sulphocyanic acid are now found in solution; the first is recognized by the prussian blue formed on adding red prusside of potassium, the second by the blood-red colour which it strikes with perchloride of iron. When nitrate of silver is added to the red-brown solution, the black precipitate already alluded to falls down, but at the same time the supernatant liquor had a reddish-brown colour; on examining this it was found to contain a persalt and protosalt of iron, the dark coloration being due to the escaping nitric oxide. The amount of sulphur precipitated during the passage of sulphuretted hydrogen through the nitroprusside is about 17 per cent.; the amount of ferrocyanide of sodium and of prussian blue has been found to vary much.

From the difficulty of obtaining the products of transformation in a pure state, I have not yet been able to make direct quantitative examinations of the various substances formed; it is therefore impossible to express the transformation in the form of an equation. From some experiments now in progress, I trust, however, to overcome those difficulties which

have prevented the completion of this study in time for the presentation of this paper.

On the Constitution of the Nitroprussides.

31. In the preceding part of the paper the analyses of the nitroprussides led to the extremely complicated formula $Fe^5 C^{24} N^{15} O^3 R^5$. This formula was *à priori* very improbable, and naturally led to the belief that an error in the estimation of the carbon forced its adoption. In fact, if 25 instead of 24 equivs. of carbon were present, the formula would resolve itself into the much simpler expression $Fe^2 C^{10} N^6 O R^2$. It is therefore important to review the evidence, in order to see whether the simple proportion of iron to carbon, 1 : 5, might be derived from it. The following table exhibits the proportion of iron and carbon found in the analyses of the respective salts:—

Name of salt.	Number of analyses furnishing the mean.	Quantity of iron. Mean.	Quantity of carbon. Mean.	Atomic relation of iron to carbon.
Nitroprusside of sodium ...	9	19·54	20·03	28 : 28·7
Nitroprusside of potassium	5	19·05	19·63	28 : 28·8
Nitroprusside of ammonium	3	22·08	22·69	28 : 28·7
Nitroprusside of silver	4	13·03	13·29	28 : 28·5
Nitroprusside of copper......	4	20·45	21·25	28 : 29·0
Nitroprusside of iron	3	19·09	19·96	28 : 29·2
Nitroprusside of zinc	1	20·07	20·53	28 : 28·6
Nitroprusside of calcium ...	1	21·09	21·47	28 : 28·5
Nitroprusside of barium......	2	14·10	14·98	28 : 29·7
Nitroprussic acid	3	23·80	24·80	28 : 29·1
Mean of the whole......	35	192·30	198·63	28 : 28·9

Now the proportion of 1 equiv. of iron to 5 equivs. of carbon would require the proportion 28 : 30. This difference is too great to be due to any errors of observation, especially when it is remembered that these, in the case of a body containing much nitrogen, tend to increase and not to diminish the apparent quantity of carbon. The actual proportion found, 28 : 28·9, indicates, in equivalents, 5 equivs. iron to 24 equivs. carbon; this proportion would require 28 : 28·8; the slight excess found is in the direction of the known errors of observation.

These considerations forced the adoption of the complex formula given above. It will also be seen, from an examination of the analytical details, that the quantity of nitrogen corresponds to 6 equivs. for every 10 equivs. of carbon, or 15 equivs. for the 24 equivs. of carbon required by the formula.

As 12 of these are in the state of cyanogen, as shown both by the transformation of the nitroprussides by alkalies and by sulphides, the remaining 3 equivs. must be in the form of an oxide of nitrogen. But the loss on the analyses does not admit the supposition that the oxide is nitric oxide, as might have been supposed, neither do the transformations countenance this idea. The oxygen is in the proportion of 3 equivs. for every 3 equivs. of nitrogen; the nitrogen not present as cyanogen must exist as nitrous oxide. This is unusual, and its functions must therefore be inquired into. It will at once be seen that if nitrous oxide is supposed to substitute and play the part of cyanogen, the iron and the non-electro-negative bodies with which it is associated are present in the same proportion as in the hypothetical radical ferrocyanogen; 5 equivs. ferrocyanogen have the formula $Fe^5 Cy^{15}$; 1 equiv. of nitroferrocyanogen has the formula $Fe^5 Cy^{12} 3NO$. The nitroprussides are therefore supposed to contain a ferrocyanogen in which 3 equivs. of cyanogen are substituted by 3 equivs. of nitrous oxide.

32. But the proportion of the electro-positive element in the nitroprussides is less than that existing either in the ferrocyanides or ferridcyanides. Liebig supposes these two latter compounds to differ by containing different radicals, one being twice the atomic weight of the other. It would be equally instructive to suppose that they both contain the same radical, but that, as in the case of the different phosphoric acids, one is quadribasic, while the other is tribasic.

Quadribasic prussides, $Fe^2 Cy^6 + 4R$, formula of ferrocyanides.

Tribasic prussides, $Fe^2 Cy^6 + 3R$, formula of ferridcyanides.

Bibasic prussides, $Fe^2 Cy^6 + 2R$, formula of undescribed compounds.

With regard to the last class, its existence must be yet considered hypothetical, but in searching for it, I have received sufficient encouragement to enable me to hope that I shall very shortly be able to establish it. Without presenting the analytical evidence to this effect, it can only be adduced as a probable hypothesis to explain the nitroprussides. The latter class of salts may be supposed to correspond to a bibasic class of prussides in which part of the cyanogen is replaced by nitrous oxide. Thus $5(Fe Cy^3 + R) = Fe^5 Cy^{15} + 5R$ correspond to 1 equiv. of a nitroprusside, $Fe^5 Cy^{12} 3NO + 5R$. The great approximation of the latter formula to the more simple expression $Fe^2 Cy^5 NO + 2R$, renders it singular that the small deficiency of carbon refuses to allow the formula to be thus

expressed. In such a case this supposed bibasic prusside and the nitroprusside would stand in a very simple relation:

$$\begin{array}{l} Fe^2\,Cy^6 \qquad +2R, \\ Fe^2\,Cy^5\,NO+2R. \end{array}$$

The complicated formula required by the analyses of all the nitroprussides might be resolved into $2(Fe^2\,Cy^5\,NO+2R)+(Fe\,Cy^2\,NO+R)$, in which the latter member is constituted on the same type, but more cyanogen is displaced by the nitrous oxide. It will not excite surprise, after what has been learned in the previous inquiry as to the obstinate manner in which the nitroprussides unite with cyanides from which they are not removable by any means tried, that a salt constituted on the same type should unite with the true nitroprussides and form an integrant conjugate compound which is not broken up by crystallization. It appears therefore very probable that the true formula of the nitroprussides may in reality be $Fe^2\,Cy^5\,NO+2R$, and that further research may eliminate this compound. Hitherto this has not been done, and the only formula which correctly expresses the analysis is $Fe^5\,Cy^{12}\,3NO+5R$, which on theoretical, but on no other grounds, may be resolved into

$$2(Fe^2\,Cy^5\,NO+2R)+(Fe\,Cy^2\,NO+R).$$

I trust soon to be able to present to the Society another memoir on the prussides, which will confirm experimentally some of the views theoretically supported in the present communication; but at present I submit the previous results with a view of drawing attention to this interesting class of salts, and with a perfect conviction that future research will simplify and explain the remarkably complex and unsatisfactory formulæ which I have been obliged to adopt, without believing them to be the correct expression of the constitution of the salts.

XLIV. *Remarks on the Weather during the Quarter ending March* 31, 1850. *By* JAMES GLAISHER, *Esq., F.R.S., F.R.A.S., and of the Royal Observatory, Greenwich**.

AT the beginning and towards the end of the quarter there was much snow; the amount of rain was less than usual. The weather was mild in February and severe towards the end of March.

* Communicated by the Author.

The mean daily temperatures of the air till January 24 were below their average values; the mean deficiency was 4°·6. From January 25 to March 13 they were above their average values; the mean excess was 5°·8; and from March 14 they were below their average values; and the mean deficiency was 6°·2.

The mean temperature of the air at Greenwich for the three months ending February, constituting the three winter months, was 39°·2; and that of the average from the seventy-nine preceding winters was 37°·6.

For the month of January was 33°·7, being 2°·0 *less* than the average of the seventy-nine preceding years, and 3°·8 *less* than that of the preceding nine years.

For the month of February was 44°·7, *exceeding* that of the average of the preceding seventy-nine years by 6°·5, and *exceeding* that of the preceding nine years by 5°·1.

For the month of March was 39°·9, being *less* than that of the average of seventy-nine years by 1°·0, and *less* than that of the preceding nine years by 2°·5.

The mean for the quarter was 39°·4, *exceeding* the average of seventy-nine years by 1°·2, and *less* than that of the preceding nine years by 0°·4.

The mean temperature of evaporation at Greenwich—

For the month of January was 32°·5; for February was 42°·3; and for March was 37°·0. These values are 4°·4 *less*, 5°·0 *greater*, and 2°·8 *less* than those of the averages of the same months in the preceding eight years.

The mean temperature of the dew-point at Greenwich—

For the months of January, February and March, were 29°·5, 39°·2 and 32°·7 respectively. These values are 5°·7 *below*, 4°·1 *above*, and 3°·9 *below* respectively the averages of the same months in the preceding eight years. The mean value for the quarter was 33°·8, and that for the preceding eight years was 35°·6.

The mean elastic force of vapour at Greenwich for the quarter was 0·214 inch, being *less* than the average from the preceding eight years by 0·023 inch.

The mean weight of water in a cubic foot of air for the quarter was 2·5 grains. The average from the preceding eight years was 2·7 grains.

The mean degree of humidity in January was 0·897, in February was 0·830, and in March was 0·770. The averages from the eight preceding years were 0·901, 0·886 and 0·836 respectively.

The mean reading of the barometer at Greenwich in January was 29·854 inches, in February was 29·828, and in March was

30·039. These readings are 0·088, 0·091, and 0·289 *greater* respectively than the averages of the same months in the preceding nine years.

The reading of the barometer at Greenwich was 30·17 inches on January 1; decreased to 29·31 by 10^{h} A.M. on the 6th; increased to 29·90 by the 13th; decreased to 29·27 by the 15th; increased to 29·86 by the 18th; decreased to 29·34 by the 19th; increased to 30·40 by the 22nd; decreased to 29·28 by 3^{h} P.M. on the 26th; increased to 30·42 by the 27th at 1^{h} P.M., having passed the point 30·00 at midnight. This great increase of 1·14 in twenty-two hours was very great. The reading on the 28th was 29·74; increased to 30·22 by the 30th; decreased to 28·80 by February 6; increased to 29·60 by Feb. 8; decreased to 29·30 by Feb. 9; increased to 30·00 by Feb. 10; decreased to 29·12 by the 12th, and increased to 30·00 by the 16th; and from this day till March 22, with the single exception of March 3, the reading was above 30 inches. This high reading for so long a period is remarkable. On March 23 the reading was 29·39, which increased to 30·01 by the 29th, and decreased to 29·67 by the end of the month.

The average weight of a cubic foot of air for the quarter, under the average temperature, humidity and pressure, was 551 grains; being six grains *greater* than the average from the eight preceding years.

The rain fallen at Greenwich in January was 1·2 inch, in February was 1·3, and in March was 0·3 respectively. The falls for these three months on an average of thirty-four years, are 1·6, 1·6 and 1·4 respectively.

The average daily ranges of the readings of the thermometer in air at the height of four feet above the soil, in January was 8°·5, in February was 11°·6, and in March was 16°·4. The averages for these three months from the preceding nine years were 8°·4, 10°·0 and 13°·4 respectively.

The minimum readings of the thermometer on grass in January was at or below 32° on twenty-four nights; the lowest was 12°·8, and exceeded 32° on seven nights; the highest was 40°·5. In February the readings were at or below 32° on sixteen nights; the lowest was 19°, and exceeded 32° on twelve nights; the highest was 44°. In March the reading was at or below 32° on twenty-four nights; the lowest was 12°·8, and exceeded 32° on seven nights; the highest was 40°.

The temperature of the Thames water, from the observations of Lieut. Sanders, R.N., Superintendent of the Dreadnought Hospital Ship, was 32°·4 in January, 41°·3 in February, and 41°·2 in March.

Fog was prevalent at Conway, Liverpool, Manchester, Birmingham, Southampton, on January 1; at Glasgow, Beattock, Lancaster, Manchester, Gloucester, Oxford, Plymouth and Southampton, on January 2; it was general on the 3rd; at Birmingham, Reading, Southampton, Brighton, Basingstoke, Hastings and Stone, on the 4th; at Liverpool, Gloucester, Oxford and Southampton, on the 6th; at Crewe, Plymouth and Manchester, on the 7th; all over the country on the 8th; at several places on the 9th; at Dundee, Liverpool, Peterborough and Gloucester, on the 10th; at Edinburgh and Southampton on the 11th; at Southampton on the 13th and 14th; at Sunderland, Whitby, Liverpool, Oxford and Southampton, on the 16th; at several places on the 17th; on the 18th it was general; on the 19th at Crewe, Southampton, Brighton and Hartwell Rectory; on the 20th at Manchester; on the 21st at Southampton; on the 22nd, 23rd, and 24th at many places; on the 25th at Folkestone and Hartwell Rectory; on the 29th at Southampton, Brighton and Hastings; on the 30th at Sunderland and Hartlepool; and on the 31st at Plymouth.

On February 1 at Hartwell, Hastings and Folkestone; on the 2nd at Weymouth; on the 4th at Plymouth, Stone and Hartwell Rectory; on the 5th at Stone; on the 15th at Weymouth, Portsmouth, Brighton and Hastings; on the 25th, 26th, 27th, it was prevalent over the south of England; and on the 28th it was general over the country.

On March 1 general in the south of England; on the 3rd at Southampton; on the 5th at Plymouth; on the 6th at Plymouth, Beckington, Basingstoke, Southampton and Stone; on the 7th and 8th it was general over the south of England; on the 9th it was general from the south coast to Hartlepool and Darlington; on the 11th at Manchester; on the 12th at Manchester, Birmingham and Plymouth; on the 13th at Birmingham, Rugby, Lancaster, Plymouth and Southampton; on the 14th at Hartlepool, Lancaster and Folkestone; on the 15th at Dundee, Lanark and Whitby; on the 16th at Whitby and Lancaster; on the 18th at Hartlepool, Whitby and Manchester; on the 19th at Hartlepool, Whitby and Conway; on the 21st at Southampton; on the 22nd at Oxford; and on the 27th at Liverpool. Fog has prevailed more or less on fifty-four days during the quarter.

Meteors were seen at Nottingham on February 3, 4, 9, 10, 11, 13, 20, 26, March 7 and 17.

At Hartwell Rectory, on February 3, a meteor was seen at 11^h P.M.; on February 9 a meteor at $11^h\ 15^m$ P.M. passed from Aries to Orion; on Feb. 11, at $10^h\ 40^m$ P.M., the bright

light of a meteor was observed, followed at an interval of two minutes by a distant report from the N.E. by N. resembling the sound which follows the distant fall of an avalanche*.

At Stone, on February 22, at $11^h\ 57^m$ P.M., a large meteor was seen to the south, which shot from west of the constellation Crater, and went southward, leaving a train of light; it exploded when near the horizon.

At Aylesbury, on February 22, at $11^h\ 45^m$, a large meteor was seen.

On March 4, at $7^h\ 20^m$ P.M., a meteor crossed Orion.

On March 11, at $8^h\ 47^m$ P.M., a meteor shot northward from east of Jupiter.

On March 15, at $7^h\ 25^m$ P.M., a meteor shot from near α Ursæ Majoris, passing Regulus, to α Hydræ.

On March 17, at $6^h\ 55^m$ P.M., a splendid meteor of the colour and size of Jupiter shot from a little above Sirius and travelled due south 15°, leaving a train of blue light about 5° in length.

On March 17, at $9^h\ 48^m$ P.M., a meteor shot from ζ Cancri, and went the short space of half a degree only.

On March 28, at $8^h\ 45^m$ P.M., a red meteor shot northward from α Persei to γ Andromedæ, leaving a train of blue light.

On March 31, at $9^h\ 5^m$ P.M., a meteor shot from γ Virginis and went east 3° or 4°.

Solar halos were seen at Nottingham on January 1; at Whitehaven on January 7; at Aylesbury on January 30; at Nottingham on February 1, 2 and 20; at Stone on March 11; at Nottingham on March 22 and 26; at Greenwich, Aylesbury, Stone and Nottingham, on March 29; and at Greenwich, Stone and Nottingham, on March 30.

Lunar halos were seen at Hartwell Rectory and Norwich on the 27th; on the 28th at Norwich; and on the 30th at Greenwich and Cardington; on February 16 at Nottingham; on the 19th at Aylesbury; on the 20th at Guernsey, Truro, Greenwich, Stone, Cardington and Norwich; on the 21st at Greenwich and Aylesbury; on the 22nd at Cardington, Norwich and Durham; on the 23rd at Guernsey, Greenwich, Hartwell Rectory, Cardington and Durham; on the 24th at Stone; on the 27th at Guernsey, Aylesbury, Stone and Hartwell Rectory; on March 21 at Hartwell Rectory and at Norwich; on March 25 at Stone and Hartwell Rectory; on the 26th at Durham and Hawarden; on March 27 at Rose Hill near

* For accounts of this meteor see the Philosophical Magazine for the months of March and April.

Oxford; on March 28 at Stonyhurst; on the 29th at Greenwich; and on the 31st at Cardington.

Lunar coronæ were seen at Stone on January 23, 25, 27, 29; March 20, 21 and 25.

Auroræ were seen on January 5 at Greenwich; on January 30 at Hartwell; on February 6 at Whitehaven, Stonyhurst and Durham; on February 9 at Manchester; on February 10 at Stone; on February 12 at Durham; on March 9 at Whitehaven; on March 10 at Nottingham and Stonyhurst; on March 11 at Greenwich and Stonyhurst; on March 27 at Rose Hill near Oxford; on March 28 at Stone; and on March 29 at Stone and Leeds.

Thunder-storms occurred on February 4 at Helston; on February 5 at Helston, Cardington and Holkham; on March 27 at Hawarden and Leeds; on March 28 and 29 at Helston.

Thunder was heard, but lightning was not seen, on January 13 at Helston; on March 28 at Derby; on March 29 at Helston; and on March 31 at Derby.

Lightning was seen, but thunder was not heard, on January 9 at Oxford; on February 4 at Leeds; on February 5 at Aylesbury, Stone, Hartwell Rectory and Nottingham; on February 6 at Aylesbury, Oxford, Hawarden and Nottingham; on February 7 at Hartwell and Hartwell Rectory; on February 8 at Hartwell; on February 9 at Leeds; and on February 27 at Hawarden.

Hail fell on January 4 at Stonyhurst; on January 5 at Guernsey, Helston and Truro; on January 6 and 8 at Holkham; on January 9 at Greenwich; on January 10 at Guernsey; on January 11 at Helston; on January 12 at Helston and Truro; on January 14 at Helston; on January 15 at Guernsey; on January 26 at Hartwell and Nottingham; on January 27 at Holkham and Rose Hill, Oxford; on February 3 at Stonyhurst; on February 5 at Uckfield, Stonyhurst and Saffron Walden; on February 6 at Guernsey, Nottingham and Stonyhurst; on February 7 at Stone, Cardington, Stonyhurst and Hawarden; on February 8 at Cardington; on February 11 at Hawarden; on February 12 at Truro and Nottingham; on March 16 at Nottingham; on March 21 at Stonyhurst; on March 23 at Helston, Truro, Hawarden and Nottingham; on March 24 at Helston, Truro and Holkham; on March 25 at Helston, Truro and Stonyhurst; and on March 27 at Helston.

Zodiacal light has been seen on February 3, 4, 7, 9, 11, 12, 13, 20, 26, and on almost every clear evening in March. Its boundary was noticed at Stone on March 4, at 7^h 10^m P.M.,

its apex reached to the Pleiades, and its north side passed by α Arietis and γ Pegasi; its south side passed by η and β Ceti. The sun seemed to be in the centre of its base; its axis was coincident with the ecliptic. The light seemed to move northward as it was going down with the stars.

Snow fell at Saffron Walden on January 3; at Manchester and Stonyhurst on the 4th; at many places on the 5th; at Uckfield, Holkham, Liverpool and Oxford, on the 6th; at Birmingham on the 7th; at Uckfield and Saffron Walden on the 8th; on every day from the 9th to the 22nd it fell at many places; at times it was falling all over the country; on the 26th it was falling at many places; and on the 31st at Shap, Darlington, Manchester, Leeds and Nottingham. On February 7 at Glasgow, Lanark and Saffron Walden; on the 12th at Glasgow, Lanark, Shap, Stone, Hartwell, Norwich, Manchester, Hawarden, Saffron Walden and Nottingham; and on the 13th at Whitby. From February 14 to March 15 no snow fell; on March 15 it fell at Hartwell; on the 17th at Holkham and Saffron Walden; on the 27th at Holkham, Leeds and York; from the 23rd to the 27th it fell at many places on every day; on the 23rd and 26th it was falling all over the country from Guernsey to Edinburgh. On the 27th at many places from the south coast to Stonyhurst; on the 28th at Derby, Leeds and Durham; and on the 29th at Leeds.

Frost at various places on January 1, 2, 5, 6, 8, 9, 10, 11, 12, 14, 15, 16, 17, 18, 21, 22, 23, 24, 29, 30, 31; February 4, 7, 12, 13; March 4, 12, 13, 18, 23, 25, 26, 27, 28 and 31.

The direction of the wind at Greenwich was south-west till January 5, passing at the rate of 86 miles daily; it was north-east from January 6 to January 21, passing at the rate of 80 miles daily; it was mostly south-west from January 21 to March 7, with an average daily motion of 155 miles; and from March 8 the direction was variable with an average motion of 70 miles.

The valuable series of observations taken at many of the railway stations, and published daily in the *Daily News*, has continued with great regularity. The working of this scheme requires the assistance of all the railways. This has been most liberally given by every company with the exception of the East Lancashire, and Lancashire and Yorkshire, which companies alone have declined to join in this work of great utility.

All the particulars received by this means and from various other sources I lay on a map daily, from which the following tables and remarks have been formed.

Jan. 1850.	Direction of the Wind.									General Remarks.
	On the south coast.	On the south-east coast.	On the east coast.	On the north-east coast.	On the north-west coast.	On the south-west coast.	In the southern counties.	In the midland counties.	In the northern counties.	
1	n.w.	n.w.	n.w.	w.n.w.	n.w.	n.w.	variable.	variable.	n.w.	Calm and fog general. Frost in several places.
2	n.w.	n.w.	w.	w.	variable.	calm.	n.w.	calm.	s.	A calm day. Fog and frost in several places.
3	n.w.	variable.	s.	s.w.	s.	s.e.	calm.	calm.	s.	Calm and fog general. [a strong breeze with rain.
4	w. & s.w.	s.w.	w.s.w.	s.w.	s.e.	w.s.w.	w. & s.w.	s. & w.	s.	Frost at Darlington. Calm general. At Glasgow and Edinburgh
5	variable.	n.w.	s.w.	w.	w.	n.w.	variable.	w.	w.	Snow extending from Edinburgh to Shap. Frost at many places
7	variable.	n.	n.w.	w.	calm.	n.w.	n.w.	n.w.	calm.	Frost general. Calm at many places with fog. [north of Rugby.
8	n.	n.	n.w.	w.	variable.	n.	n.	calm.	calm.	Fog and frost general over the country. A calm at most places.
9	n. & n.e.	n.e.	n.e.	variable.	s. & w.	calm.	n. & n.e.	calm.	calm.	Frost and snow north of Greenwich. Hail at Hastings.
10	n.	n.e.	n.e.	variable.	calm.	calm.	n. & n.e.	n.e.	w.	Frost general. Occasional snow. Wind nearly calm.
11	s.e.	s.e.	e.	s.e.	s.e.	s.e.	s.e.	s.e.	s.	Snow and frost at a few places north of Greenwich.
12	n.e. & e.	e.	e.	e.	n. & e.	calm.	n.e.	n.e.	e. & n.	Snow and sleet general to the north of Greenwich.
14	n.e.	e.	e.	e.	e.	e.	n.e.	e.	n.e.	A strong breeze everywhere. A gale at Holyhead and Yarmouth.
15	n.e.	n.e.	n.e.	e.	n.	n.e.	n.e.	n.e.	n.e. & e.	A strong breeze general. Frost and snow all over the country.
16	n.e.	n.e.	n.e.	e.	calm.	n.e.	n.e.	e.	variable.	Snow and frost general. Heavy snow at Edinburgh.
17	n.	n.	n.e.	w.n.w.	variable.	n.w.	n.	calm.	light airs.	Light snow general. Air in gentle motion, with slight fog.
18	variable.	n.w.	calm.	variable.	s. & s.e.	s.e.	variable.	s.e.	s. & s.e.	Air nearly calm. Snow and frost general. Fog.
19	n.w.	w.	variable.	s.e.	e.	s.w.	w.	variable.	variable.	Wind in gentle motion. Hard wind to the S.W. Heavy gale at
21	variable.	s.e.	s.e.	variable.	s.e.	s.e.	e. & s.e.	e. & s.e.	s.e.	Snow and frost general. Wind in gentle motion. [Exeter.
22	variable.	s.e.	variable.	variable.	variable.	s.e.	variable.	s.	calm.	Frost at a few places. Occasional snow.
23	variable.	w.s.w.	s.w.	w.	variable.	variable.	s.w. & w.	calm.	calm.	Air in gentle motion. Frost breaking.
24	calm.	w.	s.w.	variable.	variable.	calm.	light airs.	w.	light airs.	Calm and fog everywhere.
25	variable.	w.	s.w.	s.w.	variable.	variable.	s.w.	s.w.	s.	Rain general. A calm day. [53°. Snow falling in the North.
26	variable.	w.	s.w.	variable.	n.w. & w.	w.	s.w.	w.	variable.	Calm and gentle breeze to the S. Hard wind between lat. 51° and
28	s.w.	s.w.	s.w.	w. & s.w.	s.w.	s.w.	s.w.	s. & s.w.	variable.	Wind in gentle motion. Gale at Oxford. Rain at many places.
29	w. & s.w.	n.w.	n.w.	n. & n.w.	variable.	variable.	variable.	light airs.	calm.	Calm and fog general.
30	e.	s.e.	n.e.	s.	s.e. & s.	s.e.	variable.	calm.	s.e.	Frost at Darlington. Wind in gentle motion.
31	s. & s.e.	s.	s.	s.s.e.	s.	s.e.	s.e. & e.	s.e.	s. & s.e.	Snow and frost in the North. Rain and fog to the South.

Feb. 1850.	Direction of the Wind.									General Remarks.
	On the south coast.	On the south-east coast.	On the east coast.	On the north-east coast.	On the north-west coast.	On the south-west coast.	In the southern counties.	In the midland counties.	In the northern counties.	
1	s.w.	s.w.	s.	s.	s.	s.w.	s.w.	s.w.	s.	Fog at Hartlepool. Rain general.
2	s.w.	s.w.	s.w.	w.	variable.	s.w.	s.w.	s.w.	variable.	Overcast, except at Sunderland, where the sky was cloudless.
4	variable.	variable.	s.w.	variable.	variable.	variable.	s.w.	s.w.	light airs.	Calm and gentle breeze. Frost at Crewe. Partially cloudy.
5	s.w.	s.w.	w.	variable.	variable.	n.w.	variable.	n.w.	light airs.	Overcast. Rain at Cambridge. A slight frost at Darlington.
6	n.w. & w.	w.	n.w.	n.w.	n.w.	n.w.	n.w. & w.	n.w.	n.w.	Heavy gale everywhere, except on eastern side of Northern Hills.
7	n.w.	n.w. & w.	n.w.	n.w.	n.w.	n.w.	s.w.	n.w.	n.w.	A gentle breeze at most places. Sky principally clear. Snow at
8	w. & s.w.	w.	s.w.	s.w.	variable.	n.w.	s.w.	s.w.	s.	Rain falling all over the country north of Liverpool. [Glasgow.
9	s.w.	w. & s.w.	s.w.	s.w. & w.	w.	s.w.	s.w.	s.w.	s.	A heavy gale in the North. A strong wind in the South.
11	s.w.	s.w.	s.w.	s.w.	s.	s.	s.w.	s.	variable.	A hard wind at Holyhead and Bristol. Rain general.
12	s.w.	s.w.	s.w.	s.w.	w.	n.w.	s.w.	s.	s.	Rain at many places. Snow at Shap and Edinburgh. Frost at
13	n.w.	n.w.	n.w.	n.w.	n.	n.w.	n.w.	n.w.	n.w.	Frost general. Snow at Yarmouth and Whitby. [Darlington.
14	s.w.	s.w.	s.w.	s.w.	s.	s.w.	s.w.	s.	s.	Rain at many places. Snow at Shap. Strong wind to the S. Calm
15	w.	s.w.	variable.	s.w.	variable.	w.	s.w.	s.w.	variable.	Wind variable in strength. [and gentle breeze to the N.
16	w.	w.	w.	n.w.	n.w.	n.w.	n.w.	w.	w.	A calm at some places. A heavy gale in the North-west.
18	w.	w.	w.	variable.	light airs.	w.	s.w.	w.	variable.	Gentle breezes to the South. A hard wind to the North.
19	s.w.	s.w.	s.w.	s.w.	s.	s.w.	s.w.	s.w.	variable.	A hard wind at Glasgow and Edinburgh. Calm and gentle breeze
20	s.w.	s.w.	s.w.	w.	w.	s.w.	s.w.	s.w.	variable.	Rain in the S. Partially cloudy in the N. [at other places.
21	light airs.	w.	w.	w.	w.	n.w.	variable.	w.	w.	Gentle breeze in the South. A storm north of Liverpool.
22	w.	w.	w.	n.w.	n.w.	n.w.	w.	w.	n.w.	Gentle breeze in the S. A hard wind blowing from the Irish Sea
23	variable.	variable.	variable.	variable.	w.	variable.	s.w.	w.	variable.	The sky overcast. Calm or gentle breeze. [across the country.
25	variable.	variable.	variable.	s.w.	s.	s.	variable.	s.e.	variable.	Overcast. Fog at Plymouth and Hartlepool. Rain at Holyhead.
26	calm.	calm.	calm.	s.w.	s.e.	variable.	calm.	calm.	variable.	Overcast. Fog at Exeter, Hastings, Folkestone, Oxford and Whitby.
27	variable.	variable.	variable.	variable.	s.e.	s.e.	variable.	calm.	variable.	Generally overcast. Fog at many places.
28	calm.	calm.	variable.	s.w.	variable.	variable.	calm.	calm.	calm.	Calm generally. Overcast. Fog at many places.

Mar. 1850.	Direction of the Wind.									General Remarks.
	On the south coast.	On the south-east coast.	On the east coast.	On the north-east coast.	On the north-west coast.	On the south-west coast.	In the southern counties.	In the midland counties.	In the northern counties.	
1	n.	n.e.	w.	w.	s.w.	variable.	s.w.	variable.	n.w.	Fog to the South. Air in gentle motion.
2	w. & s.w.	s.w.	s.w.	s.w.	s. & s.w.	s.w.	s.w.	s.w.	s.w.	Air variable in strength, from hard wind in N. to calm in S.
4	n.w.	n.	……	n.e.	n.	n.e.	n.	n.	variable.	A heavy gale at Whitby. Frost at Lancaster. Wind strong in S.
5	n.	n.e.	w.	w.	s.w.	s.w.	variable.	s. & s.w.	s.w.	Hard wind to the N. Calm over the Southern and Midland C.
6	n. & n.e.	s.w.	n.w.	n.w.	variable.	n.w. & w.	calm.	w.	n.w. & w.	Strong westerly wind over Midland Counties. Hard wind to N.
7	variable.	calm.	calm.	w.	n.w. & w.	variable.	calm.	calm.	w.	Wind variable. Calm all over the country. Fog to the South.
8	calm.	calm.	s.w.	calm.	n.e.	n.	calm.	n.w.	w. & n.w.	General calm over the country. Fog to the South.
9	calm.	calm.	calm.	……	calm.	s.	variable.	s.w. & s.	n. & n.e.	Fog general over the country. Air in very slight motion.
11	n.e.	n.e.	n.	n.e.	n.	n.w.	variable.	n.w. & n.	n.w.	Gentle breeze to the South. Calm and fog in Midland Counties.
12	n.	n.e.	……	calm.	n.w.	n.e.	calm.	calm.	variable.	Fog in a few places. Frost at Darlington. Calm day throughout.
13	n.w.	w.	n.	s.w.	w.	s.e.	calm.	calm.	w. & n.w.	Calm general. Occasional fog. Frost at Darlington.
14	n. & n.e.	n.e.	n.w.	calm.	calm.	n.e.	variable.	calm.	calm.	A calm day. Fog in many places.
15	n.e.	n.e.	n.e.	calm.	calm.	n.e.	n.e.	n.e.	calm.	Wind in quick motion. Fog in a few places.
16	n.	w.	n.w.	n.e.	calm.	n. & s.	variable.	calm.	n. & w.	Calm in the Midland Counties. [westerly direction at N.
18	n. & n.w.	n.e.	e.	……	calm.	calm.	variable.	variable.	w.	Wind in quick motion to S. Calm in the Midland C., taking a
19	n.	n.	n.	calm.	calm.	variable.	w. & n.w.	n.w.	n.w.	Strong breeze at many places. Calm and fog to the North.
20	n.	n.e.	calm.	calm.	calm.	s.e.	n.	calm.	w.	Wind in gentle motion, increasing in strength on N.E. coast.
21	n. & n.w.	n.	n.	calm.	n.	n.e.	n. & n.e.	n. & n.e.	calm.	Slight rain to N.E. Strong breeze over the Eastern Counties.
22	variable.	n.	s.w.	……	……	s.e. & s.	n.w. & w.	n.w. & w.	n.w. & w.	Wind very variable in strength. [Whitby and Reading.
23	variable.	n.e.	n.	n.w.	n.w.	n.w.	n.w.	n.w.	variable.	Hard wind and snow general. Storms at Conway, Yarmouth,
25	n. & n.e.	n.	w.	n.w.	n.	variable.	n.w.	n.w.	n.	Air in gentle motion. Snow north of Holyhead.
26	n.w.	n.	w.	e.	e.	s.e.	w.	variable.	e.	Air variable in strength. Snow north of Bristol.
27	n.e.	s.e.	n.w.	n.w. & w.	variable.	n.e.	variable.	variable.	w.	A calm day. Snow over the whole of the country.
28	n.e.	n.	s.w.	variable.	variable.	variable.	n.w. & w.	variable.	variable.	A calm day. Frost at a few places.
29	s.e.	s.e.	s.e.	variable.	s.e.	s.e.	s.e.	s.e.	s.e. & e.	Gentle breeze at most places. Frost north of Rugby. [Whitby.
30	n.e.	s.e.	s.e.	s.e.	s.e.	s.e.	e.	s.e.	s.e.	A hard wind everywhere. Heavy gales at Crewe, Lancaster and

On January 1 the general direction of the wind was N.W. There was frost at all places, except near the south coast. Jan. 2 and 3 were mild; fog in many places. There was a gentle thaw on the 3rd. Jan. 4 was calm in many places, and light airs in others. South of latitude 52° the general direction was S.W.; between 52° and 53° it was W., and the air passed in this direction across the country, and north of this parallel it was S. Frost at Darlington only. Jan. 5 to 12 there was frequently a great diversity of direction of the wind. Frost, fog and snow were general, particularly in the eastern counties and in the north. Frequently the temperature was the lowest between the parallels of latitude of 52° and 53°. There was no frost at Guernsey. Jan. 14, there were heavy falls of snow in the northern and eastern counties. A hard wind was blowing from the N. and N.E. over the southern parts of the country, described as a gale at Yarmouth. At the same time the air was in gentle motion from the E. on the north-east coast, which on meeting the high lands in Cumberland was partly deflected up and partly down the country. The air was calm at some places in the north. Jan. 15, the direction of the wind south of latitude 53° was uniformly N.E., with a heavy wind blowing; a hard wind was blowing from the E. on the eastern side of the Cumberland mountains, and on their western side its direction was from the N. Frost everywhere, excepting Guernsey. Snow on the east coast. Jan. 16, the general direction of the wind was N.E. Snow and frost as on the 15th. Jan. 17, fog, frost and snow. Jan. 18, light airs in all directions. A hard frost, except on the south coast. Jan. 19, in the north the direction of the wind was principally E.; it was W. and S.W. between the latitudes of $51\frac{1}{4}$° and $53\frac{1}{2}$°, and it was N.W. on the south coast. A portion of the south-east coast was distinguished by a gentle wind, another by a thick fog; at the same time, from Portsmouth, round the south-west coast to Bristol, a hard wind was blowing, described as a heavy gale at Exeter. A rapid thaw everywhere. Jan. 21 to 25, the air was mostly in gentle motion. A gentle thaw set in on the 23rd, and which became general on the 25th. Jan. 26, on the south coast the air was generally calm, or in gentle motion only from the W. At Bridgewater was first felt a strong breeze from the S.W., which passed up the country, becoming stronger as it proceeded, and described as a gale at Yarmouth, and so passed to the North Sea. Above these parallels of latitude the air was mostly calm.

On Jan. 27 a heavy wind was blowing from the S. and S.W., described as a gale at Oxford, and passed over Nor-

folk to the North Sea. At Tamworth and Birmingham the air was in gentle motion only. In the north, at the same time, a strong wind was blowing from the Irish Sea, but which was not felt east of the Cumberland mountains, except at places of a considerable elevation, as at Durham. There was a rapid thaw in the south. Jan. 29 to 31, the thaw proceeded, and the air was in gentle motion.

On Feb. 1 the general direction of the wind was S.W. in the southern counties, and it was S. on the east coast and in the northern counties. Rain was falling all over the country. On Feb. 2 and 4 the general direction of the wind was S.W. There were slight frosts at Crewe and at Darlington.

On Feb. 6 there was a heavy gale from the N.W. This gale raged in Ireland and on the Welsh coast.

On a careful reference to an excellent map, and a re-examination of my note-book, containing observations on the situation of the stations made during my progress through the country for the purpose of organizing the scheme now in operation, I find that the variations, both of the strength and direction of the wind, were owing to local circumstances. Starting from the north, and confining myself to the western side of the high range of mountains, extending, with many deviations east and west, from Edinburgh to a little below Derby, I find at Dundee a gentle breeze only is recorded. This is possibly attributable to a high range of mountains, whose direction is from the S.W. to the N.E., and situated immediately above Dundee, and which would shelter it from a N.W. gale, such as that we are now investigating. At Glasgow and Lanark, places open for miles round in a N.W. direction, a storm is described. At Beattock there was a heavy gale; this place is encompassed by the Moffat hills, forming the highest ground in the south of Scotland, the highest amongst them exceeding 3000 feet above the level of the sea. Proceeding southwards, at Shap a storm was raging. This place is situated to the north of a ridge of mountains extending across the country from Whitehaven to Appleby, and from its situation is much exposed to a gale from the N.W. and W. At both Lancaster and Manchester a gale, and at Liverpool a hard wind, was recorded; the direction at these places was W. These places are open to the Irish Sea. At Conway a storm from the S. is recorded; the place is sheltered by a range of mountains to the east of it. At Holyhead there was a heavy gale from the N.W. At Crewe and Tamworth there was a heavy gale from the N.W. By reference to the map, Crewe is much exposed, and Tamworth is open from the N.N.W. Starting again from the N., on the eastern side of the mountains, at

Berwick, which is sheltered on all sides, with the exception of that open to the E., a strong breeze is recorded. At Durham and at Darlington a hard wind was recorded; but both these places, though at a considerable elevation above the sea, are protected from the N.W., at a distance, by the main ridge of mountains extending down the country. At Hartlepool, a place situated on the sea-coast, there is no mention of wind stronger than ordinary. At York there was a gale; at Whitby a strong breeze, which probably is protected by the mountains on the N.W. of it. Thus it will be seen, that, whilst a furious and destructive gale was raging on the western side of the mountains, those places on the opposite side were experiencing weather of a much more moderate character; plainly showing that the land is of sufficient elevation not only to influence the direction of the wind, but in a great measure to obstruct a storm in its progress.

A more cursory examination of the midland and southern counties will be sufficient, the hills being of sufficient elevation rarely to obstruct the wind in its course during the passage of a storm. From all those places situated near the Bristol Channel, except Exeter, including Weymouth and Guernsey, a N.W. gale is mentioned. At Exeter a strong breeze only was experienced, attributable probably to the vicinity of the Devonshire hills rising N.W. of it. The same was described at Swindon, a place situated between the range of high hills in Wiltshire and Berkshire and those in Gloucestershire. In the south-eastern counties the gale was general, but it was uniformly from the W. It is possible that this direction over a portion of the country during a N.W. gale may be owing partly to the high land in Wales, and partly to the different velocities with which the air seems to have passed down the country on the different sides of the northern mountains. At the time the observations were taken, the heaviest part of the gale had passed. It had attained its height all over the country between the hours of 3 and 6 A.M. The decrease in the reading of the barometer was great. At Chester the lowest reading occurred at $3^h\ 45^m$ A.M., and was 28·68, as observed by the Rev. A. Rigg. At Durham the lowest reading was 27·9, as observed by R. E. Carrington, Esq., being the lowest since Dec. 12, 1847. On Feb. 8 the direction of the wind was S.W. principally, but it was much deflected by the high land. Rain was falling at most places north of Holyhead. On Feb. 9 a strong S.W. wind was blowing at most places south of latitude 53°; between 53° and 55° the direction was S.; and N. of 55° it was W. A storm and heavy gale was blowing at places north of 53° 30′; and heavy rain was falling at many

places. On Feb. 11 and 12 the general direction of the wind was S.W.; rain was falling on the former all over the country, and at many places on the 12th, on which day snow fell at Edinburgh and at Shap. There was a frost at Darlington; on the 10th the wind was from the N.W., and the frost was general. Snow was falling at York and at Whitby. On the 14th the direction was S.W. and S., blowing strongly in the south, whilst the air was either calm or in gentle motion only in the north. Snow was falling at Shap, and rain was falling at many places. On the 15th the wind was very variable in strength. On the 16th the air was in gentle motion at most places, whilst a gale was blowing from the Irish Sea between the latitudes of 54° and 55°. On the 18th the air was mostly calm on the south coast, and there was a hard wind on the north-east coast. On Feb. 19 the general direction was S.W.; at places situated south of 53° 30′, and north of this parallel, it was S. At most places the air was in gentle motion. At Glasgow and Edinburgh it was blowing strongly. On Feb. 20 the wind was S.W., except in the north, where it was N.W. Rain was falling at many places in the south. On the 21st there were light airs passing in different directions; south of latitude 53° 30′ north of this parallel, a gale was blowing from the W., and which passed across the country. On the 22nd the air was in gentle motion at most places, yet there was a hard wind at some places. From this day to the end of the month the air was mostly in a calm state, and fog was prevalent.

On March 1 the air was in gentle motion in the southern parts of England, and passing for the most part from the S.W. and S., till, arriving at Beattock, Lanark and Edinburgh, a strong N.W. wind was recorded. On the 2nd the prevailing direction was S.W., veering in the northern counties to the S., or, in other words, becoming parallel to the main ridge of northern mountains; in the extreme north, the wind was blowing strongly. On the 4th the air was evidently on its return down the country, setting in at Glasgow, Lanark and Beattock from the N.W., which shortly afterwards changed to N., and continued in this direction to the south coast with scarcely an exception. At Whitby, on this day, a heavy gale from the N. is recorded. On the 5th, at Beattock, Lanark, Edinburgh and Berwick, a strong breeze was recorded, the direction being different, affording strong evidence of the effect of the mountains in deflecting the course of the wind. With a few exceptions this day was calm; the same remark applies to the 6th, with this difference, that, commencing at Holyhead, there was a fresh westerly breeze, and

the air passed in this direction over the western and midland counties, the air at the same time passing in all directions on both sides of this current, particularly on its south side, where it was mostly calm with fog. The 7th, 8th and 9th may be classed as calm days. The 10th being Sunday, I have but few observations. The 11th was calm. On the 12th the air was for the most part in gentle motion, except over the southern counties, where it was chiefly calm. The 13th and 14th days were calm. On coming to the 15th, I find there was a gentle breeze from the N.E. passing over those parts of the country extending from the south coast to Sunderland; north of this place the direction was E. Calm and fogs were registered at Lanark, Glasgow and Dundee. The temperature of the air, which till this day had been mostly above the average for the season, declined considerably below it. On the 16th the prevailing direction was N.W., but the air was deflected in many places. The temperature of this day was several degrees below its average, and at night the reading was below 25° at most places. On the 17th the direction of the wind was mostly N.E. and E. The day was very severe; its temperature at most places was 10° or 11° below the average for the season; and from this time to the end of the month the temperature of the air was low. On the 18th the air was passing in all directions, but chiefly from the N. On the 19th the directions were N.W. and N., and in many places a strong breeze was recorded. On the 20th there was a westerly current in extreme north, a N.W. wind in the northern English counties, and which dispersed in all directions in its progress to the south. On the 21st the air was passing from the N. On the 22nd the principal direction was from the W. On the 23rd there was a hard wind from the N.W., storms at some places, with snow and hail falling at others. On the 25th the direction of the wind was N.W., with a sharp frost. On the 26th the whole mass of air north of Liverpool passed from the E., and below this latitude its direction was from the N.W. On the 27th the air was in gentle motion from the E. and N.E. at southern places, and from the W. and N.W. at northern places. Snow was falling at several places in the south on the 28th, the directions were chiefly N. and N.W. On the 29th and 30th the principal directions were S.E. and E., passing with gentle motion on the former day, and there was a gale on the latter.

The mean of the numbers in the first column is 29·847 inches, and it represents that portion of the reading of the barometer due to the pressure of air; the remaining portion, or that due to the pressure of water, is 0·224 inch. The sum

Meteorological Table for the Quarter ending March 31, 1850.

The observations have been reduced to mean values, and the hygrometrical results have been deduced from Glaisher's Tables.

Names of the places.	Mean pressure of dry air at the level of the sea.	Mean temperature of the air.	Highest reading of the thermometer.	Lowest reading of the thermometer.	Mean daily range of temperature.	Mean monthly range.	Range of temperature in the quarter.	Mean temperature of the dew-point.	Wind. Mean estimated strength.	Wind. General direction.	Mean amount of cloud.	Rain. Number of days on which it fell.	Rain. Amount collected.	Mean weight of vapour in a cubic foot of air.	Mean additional weight of vapour required to saturate a cubic foot of air.	Mean degree of humidity.	Mean whole amount of water in a vertical column of atmosphere.	Mean weight of a cubic foot of air.	Height of cistern of the barometer above the level of the sea.
	in.	°	°	°	°	°	°	°					in.	grs.	grs.		in.	grs.	feet.
Guernsey	29·885	43·0	54·0	28·0	6·9	19·3	26·0	39·7	1·7	e. of n.	6·2	41	6·6	3·1	0·4	0·890	3·6	548	123
Helston	29·871	43·6	56·0	25·0	9·9	24·7	31·0	40·6	1·6	e.n.e. & s.w.	6·4	41	6·8	3·1	0·4	0·894	3·7	548	106
Falmouth	....	44·5	60·0	23·0	10·3	29·0	37·0	..	1·5	e.n.e. & w.	7·1	41	7·6						
Truro	29·947	43·3	58·0	18·0	12·1	33·0	40·0	39·2	1·0	n.e. & s.w.	6·8	38	6·9	3·0	0·5	0·856	3·5	552	35
Exeter	29·970	41·5	60·7	20·1	13·7	34·6	40·6	38·0	1·9	e. & w.	4·9	32	4·1	2·9	0·4	0·874	3·5	556	140
Chichester	....	38·7	53·0	21·0	9·0	28·7	32·0	..	..	n.n.e. & s.w.	..	..	4·4						
Uckfield	29·853	37·9	58·0	18·0	13·0	35·3	40·0	33·8	..	n.e. & w.	..	26	4·1	2·5	0·4	0·859	2·9	555	180
Southampton	29·896	40·1	60·0	10·0	8·1	32·5	50·0	35·5	0·4	......	6·4	22	4·2	2·7	0·5	0·876	3·1	553	55
Royal Observatory, Greenwich	29·882	39·4	58·0	20·0	12·2	32·4	38·0	33·8	..	......	..	25	2·8	2·5	0·5	0·832	3·0	551	159
Maidenstone Hill, Greenwich	29·904	39·2	57·6	21·5	9·8	30·6	36·1	35·5	..	n.e. & s.w.	7·3	25	2·6	2·7	0·4	0·877	3·1	553	107
St. John's Wood	29·865	39·6	60·0	20·0	13·0	33·3	40·0	34·3	1·4	......	7·0	26	2·8	2·6	0·6	0·830	3·0	551	150
Chiswell Street, London	....	41·6	57·5	25·0	8·4	24·8	32·5	37·0	..	......	..	26	2·5	2·8	0·6	0·830	3·3	549	
Latimer Rectory	29·874	37·4	56·0	10·0	14·4	35·7	46·0	32·0	1·6	n.e. & s.w.	6·9	28	3·9	1·7	0·6	0·820	3·0	549	355
Aylesbury	29·873	39·3	62·0	19·0	15·3	34·7	43·0	34·6	0·7	n.w.	7·4	29	3·1	2·6	0·4	0·850	3·1	548	284
Stone Observatory	29·859	37·6	55·0	16·9	12·9	33·6	38·1	32·9	1·1	n.e. & s.w.	6·7	26	2·4	2·4	0·5	0·845	2·9	549	320
Hartwell (near Aylesbury)	29·849	38·7	59·0	19·0	13·9	35·0	40·0	34·3	1·4	n.e. & s.w.	7·6	29	2·8	2·5	0·4	0·867	3·0	550	250
Hartwell Rectory	29·869	38·0	56·2	20·0	12·5	31·5	36·2	33·3	1·0	n.w. & s.w.	6·3	28	..	2·5	0·4	0·853	2·9	550	290
Saffron Walden	....	38·0	57·0	18·0	12·4	32·7	39·0	..	2·4	n.w.	5·6	26	2·4						
Oxford	29·865	38·7	56·5	16·7	11·5	34·1	39·8	37·3	2·1	n.e. & s.w.	7·7	20	2·4	2·8	0·2	0·940	3·4	551	210
Rose Hill, near Oxford	29·878	38·5	55·6	17·0	12·7	38·6	31·9	36·4	1·6	......	7·3	17	3·0	2·8	0·3	0·921	3·3	550	270
Cardington	29·843	38·3	57·0	19·4	12·0	31·5	37·6	34·5	1·2	......	7·1	20	1·9	2·6	0·4	0·865	3·0	553	100
Norwich	29·853	38·5	56·0	14·0	10·2	31·0	42·0	33·8	..	s.w.	7·1	32	4·5	2·5	0·5	0·815	2·9	555	23
Holkham	29·824	37·7	56·7	16·0	10·9	32·6	40·7	33·7	1·2	w. & s.w.	7·1	37	3·9	2·5	0·4	0·865	2·9	555	39
Highfield House, Notts.	29·774	38·4	61·2	17·5	14·2	34·2	43·7	34·9	1·1	Variable.	6·5	25	3·2	2·6	0·4	0·879	3·0	551	103
Derby	29·818	38·4	57·0	17·0	14·2	31·7	40·0	35·4	..	Variable.	..	32	3·4	2·7	0·4	0·874	3·1	551	140
Manchester	29·842	39·7	..	..	..	..	..	35·0	..	n.e. & n.w.	7·5	..	..	2·7	0·5	0·892	3·1	551	144
Hawarden	....	39·2	57·6	17·5	11·0	32·0	40·1	34·7	1·0	Variable.	6·6	16	3·2	2·6	0·4	0·866	3·1	548	
Liverpool	29·834	40·7	55·1	24·4	6·9	24·3	30·7	32·7	1·4	Variable.	7·5	23	3·1	2·4	0·8	0·755	2·8	552	37
Wakefield	29·796	39·0	62·0	15·5	12·1	32·8	46·5	35·7	..	w.	..	39	3·6	2·7	0·4	0·882	3·1	553	115
Leeds	29·796	38·2	59·0	20·0	10·5	29·7	39·0	35·8	1·8	n.w. & s.w.	7·4	38	3·9	2·7	0·3	0·906	3·2	561	125
Stonyhurst	29·826	37·4	54·6	16·9	11·2	30·0	37·7	34·0	1·3	w.s.w.	7·5	47	11·8	2·5	0·3	0·887	3·0	548	381
York	29·805	36·8	60·0	20·0	10·8	30·0	40·0	34·6	..	n.n.e. & s.w.	..	27	2·6		..	....	..	..	50
Whitehaven	29·754	38·8	52·5	21·5	7·1	24·7	31·0	35·9	2·2	s.w.	..	43	8·8	2·7	0·2	0·923	3·2	553	90
Durham	29·760	38·2	57·4	18·0	9·3	30·7	39·4	34·0	1·3	s.w.	5·6	34	3·6	2·6	0·4	0·865	3·0	546	340
Newcastle	29·742	39·5	55·0	21·0	10·8	29·2	34·0	35·5	..	......	..	33	5·6	2·6	0·5	0·829	3·1	550	121
Glasgow	....	37·6																	
Number of columns	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19

of these two numbers is 30·071; and it represents the mean reading of the barometer for the quarter at the level of the sea.

The mean of the numbers in the second column for Guernsey, and those places situated in the counties of Cornwall and Devonshire, is 43°·3; at Liverpool and Whitehaven is 39°·8; for those places situated south of latitude of 52°, including Chichester and Hartwell, is 38°·0; for those places situated between the latitudes of 52° and 53°, including Saffron Walden and Holkham, is 38°·3; for those places situated between the latitudes of 53° and 54°, including Derby and Stonyhurst, is 38°·4; and for Durham and Newcastle 38°·8. These values may be considered as those of the mean temperatures of the air for those parallels of latitude during the quarter ending March 31, 1850.

The average daily range of temperature in Cornwall and Devonshire was 28°·1; at Liverpool and Whitehaven was 24°·5; south of latitude 52° was 33°·2; between the latitudes of 53° and 54° was 31°·5; and north of 54° was 29°·9.

I have received the following agricultural reports:—

From Stonyhurst, favoured by the Rev. A. Weld, B.A., F.R.A.S.

The fine weather which occurred at the beginning of March induced some farmers to sow oats as early as March 15; but owing to the cold weather and severe frosts which followed, the seed has not come up as yet, and considerable fears are entertained of its failure.

Early potatoes were in some instances planted about the same time, and have not yet appeared.

Beans were sown as early as the middle of February, and are now looking very well. The severe frosts which took place about the end of March have done no harm to the fruit-trees in this neighbourhood, the vegetation fortunately not being in a sufficiently advanced state to receive any injury. Irrigation of meadows was carried on extensively in the early part of March. The severe weather which followed and the present rains have entirely suspended the working of the ground during the last fortnight. The lambing season began here about the 21st of March.

From Leeds, favoured by Charles Charnock, Esq.

As regards any remarks on agricultural matters few can be made, except that during the last three months little has been done; and the extreme coldness of March has not only checked the growth of vegetables, but the extreme frosts at night have in a great measure destroyed the blossoms of the earlier fruit-trees.

A very large quantity of potatoes are being planted in Yorkshire.

Wheat has been opportunely checked.

The land is generally in fine condition for the spring crops.

The impetus given to draining has kept the agricultural labourers well-employed during the winter.

Cattle, where healthy, have done well; but they as well as sheep have suffered from epidemics.

From Nottingham, favoured by E. J. Lowe, Esq., F.R.A.S.

Wheat looks very promising, and the grass in fields from present appearance will be early. Apricots are nearly all destroyed by frost, and gooseberries and currants injured. There is a great bloom of plums, cherries, pears and apples. Peaches and nectarines have been slightly injured by frost.

[For the monthly values of the several subjects of research, the names of the observers, and particulars of instruments used, see the Registrar-General's Quarterly Report.]

XLV. *On the Watery Secretion of the Leaves and Stems of the Ice-plant* (Mesembryanthemum crystallinum, *L.*). By Dr. Augustus Voelcker, Prof. of Chemistry Royal Agricult. College, Cirencester*.

A FEW months ago I had the pleasure of communicating to the Botanical Society of Edinburgh the results of an examination of the watery liquid in the ascidia of *Nepenthes destillatoria*. Those present at the meeting, as well as the readers of the 'Philosophical Magazine,' will remember that, in opposition to the statements of most botanists who have directed their attention to the subject of the watery secretions of the leaves of plants, I found the liquid in the ascidia of *Nepenthes* to differ materially from pure water, inasmuch as it contained from 0·30 to nearly 1 per cent. of solid substances, partly organic partly inorganic. I stated at that time my doubts as to the watery secretion of plants being nothing but pure water, and gave some reasons for this opinion; Prof. Balfour, with whom I discussed the subject, kindly furnished me with the means of investigating this point still further by favouring me with fresh specimens of the curious Ice-plant (*Mesembryanthemum crystallinum*), a plant which is remarkable on account of the gland-like vesicular eminences with which its leaves and stems are covered. The result of the examination of the fluid secreted by the leaves of this plant has fully confirmed the opinion expressed in regard to the watery secretions of plants; at all events it has shown me that the secretion of the leaves of the Ice-plant is not merely pure water, but water containing several substances in solution. Though I was unable to determine quantitatively the composition of this secretion on account of the small quantity of liquid at my command—a quantity insufficient

* Read before the Botanical Society of Edinburgh, Jan. 10, 1850.

even for a minute qualitative analysis—yet I had no difficulty in detecting the chief constituent parts of the fluid. The secretion I procured by lacerating the gland-like eminences with which the leaves are covered, with a needle, and collecting the fluid in a glass bottle. The fluid thus obtained was colourless and nearly clear, without smell, and possessing no distinctly pronounced taste. Litmus-paper dipped in it was very slightly turned red, showing the presence of merely traces of a free acid or an acid salt. In order to free it entirely from any particles of epidermis which might accidentally have mingled with the liquid, I filtered it through white filtering-paper. The fluid passing through the filter slowly was now perfectly clear. On heating to 212° F. white flakes were separated, which proved to be identical with vegetable albumen. They were collected in a filter, and the filtrate evaporated to dryness on a water-bath. During the evaporation the liquid turned yellow, particularly when evaporated to a small bulk, and left a brownish-coloured, very hygroscopic residue, which redissolved in a small quantity of distilled water, leaving but a trace of a humus-like, dark-coloured organic substance undissolved.

The chemical nature of the fluid from which the albumen had been separated, was ascertained as far as possible by the following tests:—

Ammonia produced no change.

Carbonate of ammonia gave no precipitate.

Carbonate of soda on boiling gave a white precipitate.

Oxalate of ammonia produced no change.

Phosphate of soda and ammonia, added to the concentrated liquid, gave a crystalline white precipitate of phosphate of magnesia and ammonia.

Chloride of platinum, added to the concentrated liquid after the removal of the magnesia, produced a crystalline yellow precipitate.

The presence of soda was indicated by the yellow colour given to the alcohol flame.

Lime-water produced a white precipitate.

Sulphate of lime likewise produced a white precipitate.

Chloride of barium gave a heavy white precipitate.

Nitrate of silver gave a white flaky precipitate, soluble in ammonia, but insoluble in nitric acid.

Acetate of lead produced a white precipitate.

Basic acetate of lead gave a voluminous white precipitate.

A portion of the water evaporated to dryness and heated to redness left a white ash which effervesced with acids, indicating the presence of carbonates, originated from organic acids present in the fluid.

The nature of the organic acids, which in all likelihood ac-

companied the oxalic acid, I could not determine from want of material. The presence of oxalic acid however is distinctly indicated by the above reactions. They likewise show the presence of chloride of sodium, potash, sulphuric acid and magnesia.

In comparing this secretion of the leaves of the Ice-plant with the fluid in the ascidia of *Nepenthes*, we find a material difference in their respective compositions, as will be seen by the annexed table, which exhibits the composition of both fluids:—

Composition of the fluid in the ascidia of Nepenthes.	*Composition of the watery secretion of the leaves of* Mesembryanthemum crystallinum.
Organic matter, chiefly malic and a little citric acid.	Organic matter (albumen, oxalic acid, &c.).
Chloride of potassium.	Chloride of sodium.
Soda.	Potash.
Lime.	Magnesia.
Magnesia.	Sulphuric acid.

XLVI. *On the Quaternion Expressions of Coplanarity and Homoconicism.* By WILLIAM SPOTTISWOODE, *M.A., of Balliol College, Oxford**.

THE following investigations relate to certain theorems given by Sir W. R. Hamilton in vol. xxix. of this Journal. Adopting the notation of the original papers, the equation

$$S.\alpha\alpha_1\alpha_2=0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

(where suffixes are used instead of accents) is equivalent to

$$\begin{vmatrix} x, x_1, x \\ y, y_1, y_2 \\ z, z_1, z_2 \end{vmatrix}=0, \quad . \quad . \quad . \quad . \quad . \quad . \quad (2.)$$

which may be replaced by

$$a\alpha+a_1\alpha_1+a_2\alpha_2=0; \quad . \quad . \quad . \quad . \quad . \quad . \quad (3.)$$

because, there being no linear relation between i, j, k, this last is equivalent to the system

$$\left.\begin{array}{l} ax+a_1x_1+a_2x_2=0 \\ ay+a_1y_1+a_2y_2=0 \\ az+a_1z_1+a_2z_2=0 \end{array}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (4.)$$

and (2.) or (1.) is the result of these. Hence (1.) and (3.) are alike conditions of coplanarity.

Again, if

$$\left.\begin{array}{l} \beta\ =V.V.\alpha\alpha_1.V.\alpha_3\alpha_4 \\ \beta_1=V.V.\alpha_1\alpha_2.V.\alpha_4\alpha_5 \\ \beta_2=V.V.\alpha_2\alpha_3.V.\alpha_5\alpha \end{array}\right\}, \quad . \quad . \quad . \quad . \quad . \quad (5.)$$

* Communicated by the Author.

then

$$\mathrm{V}.\alpha\alpha_1 = \begin{vmatrix} i, & x, & x_1 \\ j, & y, & y_1 \\ k, & z, & z_1 \end{vmatrix}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (6.)$$

with similar expressions the other vectors; and consequently

$$\left.\mathrm{V}.\mathrm{V}.\alpha\alpha_1.\mathrm{V}.\alpha_3\alpha_4 = (ix_1+jy_1+kz_1)\begin{vmatrix} x, & x_3, & x_4 \\ y, & y_3, & y_4 \\ z, & z_3, & z_4 \end{vmatrix} - (ix+jy+kz)\begin{vmatrix} x_1, & x_3, & x_4 \\ y_1, & y_3, & y_4 \\ z_1, & z_3, & z_4 \end{vmatrix}\right\} . \quad (7.)$$

so that

$$\left.\begin{aligned} \beta &= \alpha_1.\mathrm{S}.\alpha\;\alpha_3\alpha_4 - \alpha.\;\mathrm{S}.\alpha_1\alpha_3\alpha_4 \\ \beta_1 &= \alpha_2.\mathrm{S}.\alpha_1\alpha_4\alpha_5 - \alpha_1.\mathrm{S}.\alpha_2\alpha_4\alpha_5 \\ \beta_2 &= \alpha_3.\mathrm{S}.\alpha_2\alpha_5\alpha - \alpha_2.\mathrm{S}.\alpha_3\alpha_5\alpha \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (8.)$$

Whence, taking the product, and omitting those terms which have no scalar parts,

$$\left.\begin{aligned} \mathrm{S}\beta\beta_1\beta_2 = \mathrm{S}.\alpha_1\alpha_2\alpha_3.\mathrm{S}.\alpha\;\alpha_3\alpha_4.\mathrm{S}.\alpha_1\alpha_4\alpha_5.\mathrm{S}.\alpha_2\alpha_5\alpha \\ -\mathrm{S}.\alpha\;\alpha_1\alpha_2.\mathrm{S}.\alpha_1\alpha_3\alpha_4.\mathrm{S}.\alpha_2\alpha_4\alpha_5.\mathrm{S}.\alpha_3\alpha_5\alpha \\ +\mathrm{S}.\alpha\;\alpha_1\alpha_3.\mathrm{S}.\alpha_1\alpha_3\alpha_4.\mathrm{S}.\alpha_2\alpha_4\alpha_5.\mathrm{S}.\alpha_2\alpha_5\alpha \\ -\mathrm{S}.\alpha\;\alpha_2\alpha_3.\mathrm{S}.\alpha_1\alpha_3\alpha_4.\mathrm{S}.\alpha_1\alpha_4\alpha_5.\mathrm{S}.\alpha_2\alpha_5\alpha \end{aligned}\right\}, \quad . \quad (9.)$$

which vanishes identically whenever α coincides with any of the vectors $\alpha_1 \ldots \alpha_5$; so that these last five vectors lie on the cone represented by

$$\mathrm{S}.\beta\beta_1\beta_2 = 0,$$

when x, y, z alone are considered as variable. The only case, which is not at once obvious, is $\alpha = \alpha_4$; suppressing the common factors, (9.) then becomes

$$\left.\begin{aligned} &\mathrm{S}.\alpha\alpha_2\alpha_3.\mathrm{S}.\alpha_1\alpha\alpha_5 + \mathrm{S}.\alpha\alpha_3\alpha_1.\mathrm{S}.\alpha_2\alpha\alpha_5 + \mathrm{S}.\alpha\alpha_1\alpha_2.\mathrm{S}.\alpha_3\alpha\alpha_5 \\ &= \begin{vmatrix} x, & x_2, & x_3 \\ y, & y_2, & y_3 \\ z, & z_2, & z_3 \end{vmatrix}\begin{vmatrix} x_1, & x, & x_5 \\ y_1, & y, & y_5 \\ z_1, & z, & z_5 \end{vmatrix} + \begin{vmatrix} x, & x_3, & x_1 \\ y, & y_3, & y_1 \\ z, & z_3, & z_1 \end{vmatrix}\begin{vmatrix} x_2, & x, & x_5 \\ y_2, & y, & y_5 \\ z_2, & z, & z_5 \end{vmatrix} + \begin{vmatrix} x, & x_1, & x_2 \\ y, & y_1, & y_2 \\ z, & z_1, & z_2 \end{vmatrix}\begin{vmatrix} x_3, & x, & x_5 \\ y_3, & y, & y_5 \\ z_3, & z, & z_5 \end{vmatrix} \end{aligned}\right\}(10.)$$

(or writing

$$\lambda = yz_5 - y_5z, \quad \mu = zx_5 - z_5x, \quad \nu = xy_5 - x_5y), \quad . \quad (11.)$$

$$\left.\begin{aligned} &\begin{vmatrix} x, & x_1, & x_2, & x_3 \\ y, & y_1, & y_2, & y_3 \\ z, & z_1, & z_2, & z_3 \\ ., & (\lambda x_1+\mu y_1+\nu z_1), & -(\lambda x_2+\mu y_2+\nu z_2), & (\lambda x_3+\mu y_3+\nu z_3) \end{vmatrix} \\ &\quad = (\lambda x+\mu y+\nu z)\begin{vmatrix} x_1, & x_2, & x_3 \\ y_1, & y_2, & y_3 \\ z_1, & z_2, & z_3 \end{vmatrix} = 0 \end{aligned}\right\}(12.)$$

since $$\lambda x+\mu y+\nu z=0$$
identically.

The expression (9.) when written thus,

$$\left.\begin{matrix}S.\alpha_1\alpha_4\alpha_5.S.\alpha_2\alpha_5\alpha(S.\alpha_1\alpha_2\alpha_3.S.\alpha\alpha_3\alpha_4-S.\alpha\alpha_2\alpha_3.S.\alpha_1\alpha_3\alpha_4)\\ -S.\alpha_1\alpha_3\alpha_4.S.\alpha_2\alpha_4\alpha_5(S.\alpha\alpha_1\alpha_2.S.\alpha\alpha_3\alpha_5-S.\alpha\alpha_1\alpha_3.S.\alpha_2\alpha_5\alpha)\end{matrix}\right\}(13.)$$

may without difficulty be transformed into

$$\left.\begin{matrix}S.\alpha_1\alpha_4\alpha_5.S.\alpha_2\alpha_5\alpha.S.\alpha_2\alpha_4\alpha_3.S.\alpha_3\alpha_1\alpha\\ +S.\alpha_1\alpha_3\alpha_4.S.\alpha_2\alpha_4\alpha_5.S.\alpha\alpha_2\alpha_3.S.\alpha_1\alpha\alpha_5\end{matrix}\right\}. \quad . \quad . \quad (14.)$$

which, when equated to zero, gives the relation

$$\frac{S.\alpha_1\alpha_4\alpha_5}{S.\alpha_2\alpha_4\alpha_5}\cdot\frac{S.\alpha\alpha_2\alpha_5}{S.\alpha\alpha_1\alpha_5}=\frac{S.\alpha_1\alpha_4\alpha_3.S.\alpha\alpha_2\alpha_3}{S.\alpha_2\alpha_4\alpha_3.S.\alpha\alpha_1\alpha_3}. \quad . \quad . \quad (15.)$$

The equation (9.) expresses the property of the *Mystic Hexagram* of Pascal, and (15.) that of the *Anharmonic Ratio* of Chasles, as was explained in vol. xxix. pp. 118, 327 of this Journal.

XLVII. *On the Meteor of November* 5, 1849. *By* James Glaisher, *Esq., F.R.S., F.R.A.S., and of the Royal Observatory, Greenwich.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In the number of the Philosophical Magazine for February 1850 is a notice of a fine meteor seen by V. Fasel, Esq., F.R.A.S., at Stone, on Nov. 5, 1849. This meteor was also seen by R. L. Jones, Esq., F.R.A.S., and who wrote to me from Chester on Nov. 6, describing it. As it is seldom that a meteor can be so certainly identified as seen at two different places, I beg to send you the following particulars, in the hope that some other gentleman had the good fortune to see it and to note its path. It is possible that the meteor seen by Mr. Lowe at Nottingham on Nov. 5, at $6^h\ 20^m$, also mentioned in the same Number of the Magazine, may be the same meteor.

The following is Mr. Jones's account of the meteor:—

"I first saw it near the Pleiades, and by estimation (as I could not see the time till I got home) at $6^h\ 10^m$ P.M. G.M.T.; it passed close by α Arietis, 5° or 6° below α Andromedæ and β Pegasi, and disappeared about 10° above the four stars in the head of the Dolphin, occupying about 5° in its transit; it had a head composed of seven or eight small bluish-coloured balls, and left a vivid trace of sparks behind it. That these sparks were not the impression on the retina I am sure, as I closed my eyes, looked on the ground, and on raising my eyes again still saw them. They remained in view at least two minutes, and seemed to be attracted together in three or four masses, and the brightest part was near the meridian."

I have this day received the following additional accounts of this meteor from Mr. Fasel, who with the Rev. J. B. Reade has revisited the spot from which he saw the meteor, and found by compass that the place occupied by the meteor when first seen was due magnetic north (about 7° west of true north). Just previously to this time Mr. Fasel was walking with his face towards the west, when the bursting of the meteor caused him to turn his head towards the north. He then went home, made a diagram in his journal (a copy of which he has forwarded to me), and described it as a very brilliant meteor of the size of a star of the first magnitude, and it left a long train of red light.

From Mr. Jones's account, the meteor was first seen

At an altitude of........................	13°	and in azimuth......	68° E. of N.
Its altitude at the next observation was	27	and its azimuth......	89 E. of S.
... ...	49	...	63 E. of S.
... ...	56	...	45 E. of S.
Its altitude at explosion was	60	...	8 W. of S.

From Mr. Fasel's account, the meteor was first seen at an altitude of 30°, and in azimuth 7° W. of N., and its altitude at explosion was 38°, in azimuth 59° W. of N.

The path of the meteor seems to have been from E.N.E. to W.S.W., and was contrary to the order of planetary motion.

The intersections of the azimuths at explosion indicates that the meteor at this time was vertical over a spot at about fifteen miles from Montgomery, and north-east of it. Its distance from the earth at this time was about eighty miles.

Blackheath, April 22, 1850.

XLVIII. *Geometry and Geometers. Collected by* T. S. Davies, *Esq., F.R.S. and F.S.A.**

No. V.

THE mathematical collections of Pappus have an enduring interest to the geometer; but without question, the short notice respecting the Porisms of Euclid (in the preface to his seventh book) is the most interesting part of the whole work.

It is unknown what MSS. were used by Commandine in making his translation; and all that have been since discovered are of comparatively modern dates. From some remarkable coincidences, there is reason to believe that the MS. of Commandine was of the same period as some of these:—one of which coincidences is, the absence of a figure alike from all known codices and from Commandine's translation. They are all alike defective in not even verbally describing this

* Communicated by the Author.

figure*. Other and multiplied coincidences might be pointed out, all of which tend to show that the existing MSS. are only copies (with sometimes unaccountable variations, it is true) from some one of an earlier date,—and this, too, an imperfect transcript of the original.

The Savilian Library of Oxford contains two such MSS. (Nos. 3 and 9); and from these Halley attempted to form a text of that celebrated preface. This was prefixed to his Restoration of the Section of Ratio and Section of Space, printed in 1706; and is still the only text that is considered to make any probable approach towards the original. Yet he gives up in despair all attempts to elucidate what is said respecting the Porisms.

Under these circumstances it would have been advisable (at least as regards the section, *Περὶ τῶν πορισμάτων Εὐκλείδου*, pp. vi.–ix.) to give the readings of each of his MSS., and his reasons for any departure from them, however slight. The

* M. Breton, indeed, denies that this figure (or these figures) has ever existed, or was at all necessary (*Comptes Rendus*, Oct. 29, 1849, p. 482): but in this he stands alone, and offers a very unsatisfactory reason,—"puisqu'il s'agissait de propriétés générales." As, however, M. Breton's paper is not published, and only a slight notice given of it in the place referred to, it would be obviously an inappropriate subject of comment here. Still his general views are indicated with precision; and I may be (especially as my thoughts are deeply occupied with the same subject) permitted to express my entire dissent from his conclusions, and my conviction that his interpretation is alike contradictory to all historical evidence, and incompatible with the state of geometrical science in Euclid's time.

I hope to be able to offer conclusive evidence that the porism of Euclid must have been what Simson divined it to be, and *could not possibly have been anything else*. I speak of it as a proposition: but I do not venture to affirm that any single actual porism that has been said to be "restored" is precisely one of those which Euclid gave, whether offered as such by Simson, Noble, or whoever else has made the attempt. They *may be* so: but so long as the same lemma may be subservient to many porisms, how are we to tell which of these many was really Euclid's individual porism? Probability would indeed rest upon the simplest, but certainty upon no one.

The fragmentary character of the description of the several porisms in the last three paragraphs of the text renders it impossible to affirm whether the words refer to the predicate of the proposition, or to successive mutations of the subject; or, in other words, whether they express the conclusion in which the reasoning is to terminate, or the several interchanges of one single condition. The former has been uniformly assumed: but, when closely examined, it seems to lead to difficulties from which the latter is free. One of these difficulties is, that it is impossible to see in an enunciation so constructed anything more than a local theorem. This assumption, indeed, may account for the almost universal tendency (especially amongst the continental geometers) to confound the porism with the local theorem. There is no question, however, that the propositions given by Simson, as porisms, are truly and essentially such, whether they coincide with any of Euclid's or not.

I cannot, however, enter into further details here. What I have to offer on the subject will hence be reserved for another occasion and another place.

reasons can now, of course, only be conjectured from the alterations which he made: but it only requires careful collation to assign what those changes were. I am fortunately enabled to give these from a MS. of the late Mr. S. P. Rigaud, Savilian Professor of Geometry in the University of Oxford, drawn up in 1815, 1816, and undertaken at the request of the late Professor Leybourn of the Royal Military College, Sandhurst. I believe that all who had the privilege of Professor Rigaud's friendship will admit that he was, from his habitual care and accuracy, eminently likely to observe even the most minute variation:—they would trust his eye quite as much as their own, and feel even more confidence in his transcripts than if made by themselves. This collation, too, was made three times over; and by a scholar resident on the spot, who could examine the MSS. at his leisure, and who moreover thoroughly understood the subject to which they relate.

It was for the use of Mr. Mark Noble, one of the then Sandhurst staff of Professors, that Mr. Leybourn procured this collation. Mr. Noble had some years previously published two short papers on Porisms, into the analysis of which he conceived he had introduced material improvements. [See Leybourn's Mathematical Repository, vol. i. p. 35, N.S., and Gentleman's Mathematical Companion, vol. ii. p. 42.] That venerable mathematician placed the papers in my hands a short time ago, with the hope that they might be of use to Mr. Potts and myself in preparing our notes to the translation of Simson's Porisms; and, knowing the casualties to which loose papers are liable when they are vested in private families, I have thought it better to secure for the public a correct copy in print, whilst the MS. remains in my possession. It will save, too, a good deal of trouble, and remove in some degree the vagueness with which we all estimate the value of information which is not generally accessible. If, too, other MSS. (the Paris and Burney, for instance) were collated with the same care, and printed in the same manner, all the information that *literature* can supply on this subject would be open to all; instead of being, as now, confined to a few, and obtained even by them with much trouble and expense.

Any notes made by Simson in his copy of Commandine would also tend to utility. The *Adversaria*, too, might contribute something, were those valuable papers carefully examined. Glasgow, however, has her own resident geometers; and it is not too much to hope, that regard for her own honour, and veneration for her great men of past days, will operate as sufficient incentives to this illustration of a book so intimately connected with Simson's and with Scotland's fame.

A short summary of the contents of the mathematical col-

lections was given by Dr. Hutton in the first edition of his Dictionary (1796); and a somewhat more philosophical one by Dr. Trail in the third supplement to his Life of Simson (1812).

The letter itself is sufficiently explicit as to the signification of the references and construction of the list of readings. Those which were crossed out are here dotted beneath, to still guide the reader to the words which were originally noted; and the numbers referring to them are omitted from the margin. Those words where the variations are retained have a line beneath them in the original; but are here referred to by numbers and letters in the usual manner.

"*MS. Sav.* 3. The propositions are not numbered on from the beginning of the book, but have a separate numbering for each part of the 7th Book.—What is Prop. 127 of Commandine stands thus in the manuscript:—

Πορισμάτων α^ον β^ον γ^ον.

Του πρῶτου εἰς τὸ πρῶτον πορίσμα ἔστω καταγραφὴ ᾱ
ἡ αβγδεζη και ἔστω ὡς ᾱζ πρὸς τὴν ζη̄, οὕτως ἡ ᾱδ πρὸς κζ
τὴν δγ̄, καὶ ἐπεζεύχθω ἡ θκ̄ οτι παράλληλός ἐστιν ἡ θκ̄,
(N. the enunciation ends in this incomplete manner).

"What Commandine has in p. 355 (Ed. 1660), 'Per compositam vero proportionem, &c.' is marked *β* in the margin of MS., and Commandine's Prop. 128 begins thus:

Εἰς τὸ δεύτερον πόρισμα καταγραφὴ ἡ αβ̄ γδ̄ ε̄ζ η̄θ γ̄
ἔστω δε παράλληλος ἡ ᾱζ τῇ ᾱβ ὡς δε ἡ ᾱε πρὸς τὴν ε̄ζ
οὕτως ἡ γ̄η πρὸς την κ̄ζ. ὅτι εὐθειά ἐστιν ἡ διὰ τῶν θκ̄ζ.

"Commandine's 164 begins thus:

Εις το πορισμα ᾱ βιβλιου.

Θέσει ὄντος παραλληλογράμμου του ᾱδ ἀπὸ δοθέντος ᾱ
του ε̄ διαγραφειν τὴν ε̄ζ καὶ ποιειν ἴσον τὸ ζγ̄η τρίγωνον
τῳ ᾱδ παραλληλογράμμῳ.

"*MS.* 9. *Πορισματων ᾱ β̄ γ̄.*

του πρωτου εις το α^ον πορισμα.

Εστω καταγραφη η αβγδεζη και εστω ως η αζ προς την ζη̄ ουτως η ᾱδ προς την δγ̄ και επεζευχθω η θκ̄ οτι =^os εστιν η θκ̄ τη αγ.

Εις το δευτερον πορισμα καταγραφη η αβ̄ γδ̄ ε̄ζ εστω δε =^os η ᾱζ τη δβ̄. ως δε η ᾱε προς την ε̄ζ ουτως γ̄η προς την η̄ζ οτι ευθεια εστιν η δια των θκ̄ζ.

Εις το πορισμα του ᾱ βιβλιου.

Θεσει οντος =^γρου του ᾱδ απο δοθεντος του ε̄ διαγαγειν την ε̄ζ και ποιειν ισον το ζγ̄η Δ^ον τω ᾱδ =^γρω.

"Sir, "Oxford, Feb. 24, 1816.

"The above are all the instances in which I can find anything like what you desired me to look for: but although they are only three in number, the last is new, as I see nothing like it in Commandine. I am much obliged to you for Beja Ganita, and regret that your efforts for the promotion of scientific literature should be so little profitable to you. I have had the pleasure of being able to complete the collation sooner than I expected, and I lose no time in forwarding it to you. I hope your friend will have no difficulty in understanding the references which I have made in the margins. You may explain to him that I have drawn lines under the words to which any various readings occurred, and I have put numbers in the margins referring to them. These will be found to have two interruptions: the first is from my having in a few instances discovered variations in a second collation which had previously escaped me, such as (18) No. 3. P. VII. (28) No. 9. P. VII. and (29) No. 9. P. IX.; the other interruption is from my having erased many references which I had made in my first collation. These erasures consisted almost entirely of what had been introduced by an excess of minuteness, which had induced me to copy out some of the contractions. Hence you will find very many of them in P. VIII., for in the MS. No. 9 the page which contains from *δαψιλεστερον* (line penult. P. VII.) to *δοθεντα* (line 4. P. IX.) is written in a different hand from the two pages which precede and follow it, and it contains several contractions which differ from the characters used in them; thus (29) it has *δεδομεν⁻σηων* for *δεδομενων σημειων* where the printed text has *δεδομενω σημειω*; but as I had no doubts about the meaning of any of these contractions, I thought it useless to leave the mention of them where no variation was offered in the readings. Wherever any erasure has been made, the line which was drawn under the word to which the reference was made is marked thus . . . . . In some cases there are more words in the margin than that to which the various reading belongs; but this occurs only where I thought that there might otherwise be some mistake: if, however, in this or in any other particular I have failed in making myself clearly understood, I shall be happy in hearing from you or from your friend (although I possibly have not the pleasure of his acquaintance), and as I have Halley's book at hand, it will only be necessary to mark the page and line in which the difficulty may occur.

"I have carefully made the collation three several times, and the last time I repeated it I could discover no variation which I had not previously noted; I therefore feel confident of the accuracy of what I send you. I hope likewise that it

is sufficiently minute, as I have not omitted to mark even what are most obviously the blunders of transcription. I did not, however, think it necessary to set down where words were differently divided from what they are in the printed text, and which in many instances occurs in No. 9 without any reason, and I gave up any attention to accents in that MS.; indeed you may remember what Wallis says, 'accentuum spirituumque notæ et interstinctionum puncta desiderantur fere omnia.' He certainly studied these MSS. with great attention; but still I cannot feel perfectly satisfied with his conjecture of the one being only a copy of the other. Readings and omissions likewise will be found in each which do not occur in the other; and though there are very many instances of peculiar similarity in them, this may have been occasioned by their both being transcripts from the same original.

"Pray remember me to my old acquaintance Mr. Dalby, whom by some strange oversight I neglected to mention in my last letter. "Your most humble Servant,

"Oxford, Feb. 24, 1816." "S. P. RIGAUD."

"P.S. You omitted to mention the bookseller who receives the subscription for the Diaries. I shall go to the publisher if I hear nothing from you to the contrary; and I should not have troubled you on the subject, if I had not been afraid of a mistake between your copies (for one of which I consider myself as subscribing) and those which remained as Mr. Mawman's property."

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[1]*Περὶ τῶν πορισμάτων Εὐκλείδου*[a].

Μετὰ δὲ τὰς ἐπαφὰς ἐν τρισὶ βιβλίοις πορίσματά ἐστιν Εὐκλείδου, πολλοῖς ἄθροισμα φιλοτεχνότατον εἰς τὴν ἀνάλυσιν τῶν ἐμβριθεστέρων προβλημάτων καὶ τῶν γενῶν, [2]*ἀπερίληπτον*[b] *τῆς φύσεως παρεχομένης πλῆθος. Οὐδὲν προστεθείκασι τοῖς ὑπ'*[c] *Εὐκλείδου γραφεῖσι πρώτου, χωρὶς εἰ μή τινες τῶν πρὸ ἡμῶν ἀπειρόκαλοι δευτέρας γραφὰς ὀλίγοις αὐτῶν παρατεθείκασιν·*[d] *ἑκάστου μὲν πλῆθος*

References to MS. No. 3.

[1] There is no such title in the MS., it is written thus:
πτωσιν:
ἔχει δὲ τὸ πρωτον των ἐπαφων προβλήματα $\bar{\zeta}$.
τὸ δὲ δεύτερον προβλήματα $\bar{\delta}$
Λήμματα δέ ἔχει τὰ β'· βιβλία, κδ. αὐτά δὲ θεωρημάτα ἐστίν $\bar{\xi}$. *μετὰ δέ τὰς ἐπαφὰς. κ.τ.λ.*

εχει δε, &c. to Λ inclusive is written in red ink, the rest is in black.

In the margin is "Euclidis Porism. Lib. 3."

[2] *υπο.*

References to MS. No. 9.

[a] There is no title in the text, but in the margin there is *πορισματά* 3 Lib.—There is no distinction of red and black in it, but there is a break in the line thus: *κατα πτῶσιν,*
εχει δε τὸ πρωτον τῶν επαφῶν προβλημὰτα επτα, το δε δευτερον προβληματα τεσσαρα. λημματα δὲ ἐχέι τὰ δυό βιβλια κα'. αυτὰ δε θεωρηματων εστιν $\bar{\xi}$. *μετὰ δε τας επαφὰς κ.τ.λ.*

[b] *απεριληπτου.*

[c] *ὑπο.*

[d] *παρατεθεκασι.*

[1]ὡρισμένον[a] ἔχοντος ἀποδείξεων, * ὡς ἐδείξαμεν, [2]τοῦ δὲ Εὐκλείδου[b] μίαν ἑκάστου θέντος τὴν μάλιστα [3]ὑπεμφαίνουσαν[c]. Ταῦτα[d] δὲ λεπτὴν καὶ φυσικὴν ἔχει θεωρίαν καὶ ἀναγκαῖαν καὶ καθολικωτέραν, καὶ τοῖς δυναμένοις[e] ὁρᾶν καὶ πορίζειν ἐπιτερπῆ. ἅπαντα δὲ αὐτῶν τὰ εἴδη οὔτε Θεωρημάτων [4]ἐστὶ οὔτε προβλημάτων, ἀλλὰ μέσην πως[f] τούτων ἐχούσης ἰδέας· ὥστε τὰς προτάσεις αὐτῶν δύνασθαι σχηματίζεσθαι [5]ἢ ὡς[g] θεωρημάτων ἢ ὡς προβλημάτων· παρ' ὃ καὶ συμβέβηκεν,[h] τῶν πολλῶν γεωμετρῶν τοὺς μὲν ὑπολαμβάνειν αὐτὰ εἶναι τῷ γένει[i] θεωρήματα, τοὺς δὲ προβλήματα, ἀποβλέποντας τῷ σχήματι μόνον τῆς προτάσεως. [6]τὰς[j] δὲ διαφορὰς τῶν τριῶν τούτων ὅτι βέλτιον [7]ᾔδεισαν[k] οἱ ἀρχαῖοι, δῆλον ἐκ τῶν ὅρων. ἔφασαν γὰρ θεώρημα μὲν εἶναι τὸ προτεινόμενον[l] εἰς [8]ἀπόδειξιν αὐτοῦ τοῦ προτεινομένου· πρόβλημα δὲ τὸ προβαλλόμενον εἰς κατασκευὴν αὐτοῦ τοῦ προτεινομένου· πόρισμα δὲ τὸ προτεινόμενον εἰς πορισμὸν[m] αὐτοῦ τοῦ προτεινομένου. μετεγράφη[n] δὲ οὗτος ὁ τοῦ πορίσματος ὅρος ὑπὸ τῶν νεωτέρων, μὴ δυναμένων ἅπαντα πορίζειν, ἀλλὰ συγχρωμένων τοῖς στοιχείοις τούτοις, καὶ δεικνύντων αὐτὸ μόνον τοῦθ' ὅτι ἐστὶ τὸ ζητούμενον, μὴ ποριζόντων δὲ τοῦτο· καὶ [9]ἐλεγχόμενοι[o] ὑπὸ τοῦ ὅρου καὶ τῶν διδασκομένων[p], [10]ἔγραψαν[q] ἀπὸ συμβεβηκότος οὕτως[r]. πόρισμά ἐστι τὸ λεῖπον ὑποθέσει [11]τοπικοῦ θεωρήματος. τούτου δὲ τοῦ γένους τῶν πορισμάτων εἶδός ἐστιν οἱ τόποι, καὶ [12]πλεονάζουσι ἐν τῷ ἀναλυομένῳ· κεχωρισμένων δὲ τῶν πορισμάτων ἤθροισται καὶ ἐπιγράφεται καὶ παραδίδοται διὰ τὸ πολύχυτον εἶναι μᾶλλον τῶν ἄλλων εἰδῶν. τῶν γοῦν τόπων [13]ἐστιν[s] ἃ μὲν ἐπιπέδων, ἃ δὲ στερεῶν, ἃ δὲ γραμμικῶν, καὶ [14]ἔτι[t] τῶν πρὸς μεσότητας. Συμβέβηκεν[u] δὲ καὶ τοῦτο τοῖς πορίσμασι, τὰς προτάσεις ἔχειν ἐπιτετμημένας διὰ τὴν σκολιότητα πολλῶν συνήθως συνυπακουομένων, ὥστε πολλοὺς τῶν γεωμετρῶν ἐπὶ μέρους[v] [15]ἐκδέχεσθαι[w], τὰ δὲ ἀναγκαιότερα ἀγνοεῖν τῶν σημαινομένων. περιλαβεῖν δὲ πολλὰ μιᾷ προτάσει [16]ἥκιστα[x] δυνατὸν ἐν τούτοις, διὰ τὸ καὶ αὐτὸν Εὐκλείδην οὐ πολλὰ ἐξ ἑκάστου εἴδους τεθεικέναι, ἀλλὰ δείγματος ἕνεκα τῆς [17]πολυπληθίας[y] * ἐνῇ ὀλίγα πρὸς ἀρχήν. δεδομένον * τοῦ πρώτου[z] βιβλίου τέθεικεν ὁμοειδῆ πᾶν ἐκείνου τοῦ δαψιλεστέρου εἴδους τῶν τόπων, ὡς δέκα * τὸ πλῆθος. Διὸ καὶ περιλαβεῖν ταύτας ἐν[1a] μιᾷ προτάσει ἐνδεχόμενον εὑρόντες[1b] οὕτως ἐγράψαμεν.

References to MS. No. 3.	References to MS. No. 9.
1 *ὡρισαμένου.*	a *ορισμενον.*
2 *τὴν δ' εὐκλειδου.*	b *την δε ευκλειδου.*
3 *απεμφαινουσαν.*	c *απεμφαινουσαν.*
	d *ταυτην.*
	e *δυνομενοις.*
4 *εστιν.*	f *επι.*
5 *τεώς.*	g *τεως.*
	h *συμβεβηκε.*
6 *τῆς.*	i *τω γενει αυτα ειναι.*
	j *την.*
7 *ἤδεσαν,*	k *ηδεσαν.*
8 *ἀπόδειισιν.*	l *προβαλλομενον* and *τειν* in margin.
	m *πορισμα.*
	n *μεταγραφη.*
9 *ἐλεγχομένων.*	o *ελεγχομενων.*
10 *ἔγραψαν δὲ ἀπὸ.*	p *τωνιδιδασκομενων.*
11 *τυπικου.*	q *εγραψαν δε απο.*
12 *πλεονάζουσιν.*	r *ουτω.*
13 *ἐστὶ ῑ ἇ.*	s *εστι δεκα α.*
14 *επι* for *ετι.*	t *επι.*
	u *συμβεβηκε.*
	v *μερου.*
15 *εκδεχεται.*	w *εδεχεται.*
16 *ἥδιστα.*	x *ηδιστε.*
17 *πολυπλάθιάς.*	y *πολυπληθι* and the two last letters of the word are blotted.
	z *προυτου.*
	1a *εν* not in MS.
	1b *ορωντες.*

Ἐὰν ὑπτίου ἢ παρυπτίου [1]ἢ παραλλήλου * ἑτέρᾳ τρία τὰ ἐπὶ μιᾶς σημεῖα[a] δεδομένα ᾖ, τὰ δὲ λοιπὰ πλὴν ἑνὸς ἅπτηται θέσει δεδομένης εὐθείας, καὶ τοῦθ' ἅψεται θέσει δεδομένης εὐθείας. τοῦτ' [2]ἐπὶ[b] τεσσάρων μὲν εὐθειῶν εἴρηται μόνων, ὧν οὐ πλείονες ἢ δύο διὰ τοῦ αὐτοῦ σημείου εἰσίν· ἀγνοεῖται δὲ ἐπὶ παντὸς τοῦ προτεινομένου πλήθους ἀληθὲς ὑπάρχον οὕτω λεγόμενον. ἐὰν ὁποσαιοῦν εὐθεῖαι τέμνωσιν ἀλλήλας [3]μὴ[c] πλείονες ἢ δύο διὰ τοῦ αὐτοῦ σημείου, πάντα δὲ ἐπὶ μιᾶς αὐτῶν δεδομένα ᾖ, καὶ τῶν ἐπὶ ἑτέρας ἕκαστον ἅπτηται θέσει δεδομένης εὐθείας· ἢ καθολικώτερον[d] οὕτως, ἐὰν ὁποσαιοῦν εὐθεῖαι τέμνωσιν ἀλλήλας μὴ πλείονες ἢ δύο διὰ τοῦ αὐτοῦ σημείου, πάντα δὲ τὰ ἐπὶ μιᾶς αὐτῶν [4]σημεῖα[e] δεδομένα ᾖ, τῶν δὲ λοιπῶν τὸ πλῆθος ἐχόντων τρίγωνον ἀριθμὸν, ἡ πλευρὰ τούτου ἕκαστον ἔχει σημεῖον ἁπτόμενον εὐθείας θέσει δεδομένης, [5]ὧν τριῶν μὴ πρὸς γωνίαν ὑπάρχον τριγώνου χωρίου ἕκαστον λοιπὸν σημεῖον ἅψεται θέσει δεδομένης εὐθείας. τὸν δὲ στοιχειωτὴν[f] οὐκ εἰκὸς ἀγνοῆσαι τοῦτο, τὴν δ'[g] ἀρχὴν μόνην τάξαι. καὶ ἐπὶ πάντων δὲ τῶν[h] πορισμάτων φαίνεται ἀρχὰς καὶ σπέρματα μόνα[i] πληθῶν πολλῶν καὶ μεγάλων [6]καταβεβληκέναι, ὧν [7]ἕκαστον οὐ κατὰ τὰς τῶν ὑποθέσεων[j] διαφορὰς διαστέλλειν δεῖ, ἀλλὰ κατὰ τὰς τῶν συμβεβηκότων καὶ ζητουμένων· αἱ μὲν ὑποθέσεις ἅπασαι [8]διαφέρουσιν[k] ἀλλήλων εἰδικώταται οὖσαι, τῶν δὲ συμβαινόντων καὶ ζητουμένων [9]ἕκαστον ἓν καὶ τὸ αὐτὸ ὂν πολλαῖς [10]ὑποθέσεσιν[l] διαφόροις [11]συμβέβηκε[m].

Ποιητέον οὖν ἐν μὲν τῷ πρώτῳ βιβλίῳ ταῦτα τὰ γένη τῶν ἐν ταῖς προτάσεσι ζητουμένων. (ἐν ἀρχῇ μὲν [12]τοῦ ζʹ διάγραμμα τοῦτο). Ἐὰν ἀπὸ δύο δεδομένων σημείων πρὸς θέσει δεδομένην εὐθεῖαν [13]κλασθῶσιν[n], ἀποτέμνῃ δὲ [14]μία[o] ἀπὸ θέσει δεδομένης εὐθείας[p] πρὸς τῷ ἐπ' αὐτῆς δεδομένῳ σημείῳ[q], ἀποτεμεῖ καὶ ἡ ἑτέρα ἀπὸ ἑτέρας λόγον [15]ἔχουσαν[r] δοθέντα. ἐν δὲ τοῖς ἑξῆς, ὅτι τόδε τὸ σημεῖον ἅπτεται θέσει δεδομένης εὐθείας· ὅτι λόγος τῆσδε πρὸς τήνδε δοθείς· ὅτι λόγος τῆσδε πρὸς ἀποτομήν· ὅτι ἥδε θέσει δεδομένη ἐστίν· ὅτι ἥδε ἐπὶ δοθὲν νεύει· [16]ὅτι λόγος τῆσδε πρός τινα ἀπὸ τοῦδε [17]ἕως[s] δοθέντος· ὅτι λόγος τῆσδε πρός τινα ἀπὸ τοῦδε [18]κατηγμένην·[t] ὅτι λόγος τοῦδε τοῦ * χωρίου [19]πρὸς τὸ ὑπὸ δοθεί-

References to MS. No. 3.

1 τρία τὰ ἐπὶ μιᾶς σημειον ἢ παραλλήλου ἕτερα τα.

2 εστι.

3 μη is not in MS.

4 σημειων.

5 ᾧ.

6 καταβεβλημενας.

7 ἐν ῆ for εκαστον.

8 διαφυρουσιν.

9 εκαστην.

10 υποθεσεσι.

11 συμβέβηκε τῳ ταῦτα γένη.

12 τό ζ.

13 κλαθωσιν.

14 μιαν.

15 εχουσα.

16 ὁ λόγος.

17 ὡς.

18 κατηγμενης.

19 πρὸς τὸ ὑπὸ δοθείσης καὶ τῆσδε· ὅτι τοῦδε τοῦ χωρίου not in MS.

References to MS. No. 9.

a τρια τα επι μιας $\sigma\eta^{o\upsilon}\bar{\eta} = {}^{o\upsilon}$ ετερα τα.

b εστι.

c μη not in MS.

d καθωλικωτερον.

e $\sigma\eta^{\omega\nu}$.

f του δε στοιχειωτου.

g δε.

h των not in MS.

i μονὂν both in same hand and ink.

j καταβεβλημενὰς ὧν ἐν ῆ οὐ κατα τῶν υποθεσεων.

k διαφορουσιν.

l υποθεσεσι.

m συμβεβηκε τω, ταυτα γένη ποιητέον οὖν ἐν μεν.

n κλαθωσιν.

o μιαν.

p ευθειας not in MS.

q δεδομεν̂ $\sigma\eta^{\omega\nu}$.

r εχουσα.

s ως.

t κατηγμενης.

σης καὶ τῆσδε· ὅτι τοῦδε τοῦ χωρίου ὁ [1]μὲν τὶ δοθέν ἐστιν, ὁ δὲ λόγον ἔχει πρὸς ἀποτομήν· ὅτι τόδε τὸ χωρίον, ἢ τόδε μετά τινος χωρίου δοθέντος ἐστὶν, * ἐκεῖνο δὲ λόγον ἔχει πρὸς ἀποτομήν· ὅτι ἥδε μεθ' ἧς πρὸς ἣν ἥδε λόγον ἔχει δοθέντα, λόγον ἔχει πρός τινα ἀπὸ τοῦδε [2]ἕως[a] δοθέντος· ὅτι τὸ ὑπὸ [3]τοῦ δοθέντος καὶ [4]τῆσδε, ἴσον[b] ἐστὶ τῷ ὑπὸ [5]δοθέντος[c] καὶ [6]τῆς[d] ἀπὸ τοῦδε ἕως δοθέντος· ὅτι λόγος τῆσδε καὶ τῆσδε πρός τινα ἀπὸ τοῦδε [7]ἕως[e] δοθέντος· ὅτι ἥδε ἀποτέμνει ἀπὸ θέσει δεδομένων δοθὲν περιεχούσας.

Ἐν δὲ τῷ δευτέρῳ βιβλίῳ ὑποθέσεις μὲν ἕτεραι, τῶνδε ζητουμένων τὰ μὲν πλείονα τὰ αὐτὰ τοῖς ἐν τῷ πρώτῳ βιβλίῳ, περισσὰ δὲ ταῦτα· ὅτι τόδε τὸ χωρίον ἤτοι λόγον ἔχει πρὸς ἀποτομὴν, [8]ἢ[f] μετὰ δοθέντος λόγον ἔχει πρὸς ἀποτομήν· ὅτι [9]λόγος[g] τοῦ ὑπὸ τῶνδε πρὸς ἀποτομήν· ὅτι λόγος τοῦ[h] ὑπὸ συναμφοτέρου τῶνδε καὶ * [10]συναμφοτέρου[i] τῶνδε πρὸς ἀποτομήν· ὅτι τὸ ὑπὸ τῆσδε καὶ [11]συναμφοτέρου τῆσδέ τε καὶ τῆς πρὸς ἣν ἥδε λόγον ἔχει δοθέντα· καὶ τὸ ὑπὸ τῆσδε καὶ τῆς πρὸς ἣν ἥδε λόγον ἔχει δοθέντα, λόγον ἔχει πρὸς ἀποτομήν· ὅτι [12]λόγος[j] συναμφοτέρου πρός τινα [13]ἀπὸ[k] τοῦδε ἕως δοθέντος· ὅτι δοθὲν τὸ ὑπὸ τῶνδε.

Εν δὲ τῷ τρίτῳ βιβλίῳ αἱ μὲν πλείονες ὑποθέσεις ἐπὶ ἡμικυκλίων εἰσὶν, ὀλίγαι[l] δὲ ἐπὶ κύκλου καὶ τμημάτων· τῶν δὲ ζητουμένων τὰ μὲν πολλὰ παραπλησίως τοῖς ἔμπροσθεν, περισσὰ δὲ ταῦτα· ὅτι λόγος τοῦ ὑπὸ τῶνδε πρὸς [14]τὸ ὑπὸ τῶνδε·[m] ὅτι λόγος τοῦ ἀπὸ τῆσδε πρὸς ἀποτομήν·[n] ὅτι τ` ὑπὸ τῶνδε τῷ ὑπὸ δοθείσης[o] καὶ [15]τῆς[p] ἀπὸ τοῦδε ἕως δοθέντος· ὅτι τὸ ἀπὸ τῆσδε τῷ ὑπὸ δοθέντος καὶ ἀπολαμβανομένης ὑπὸ καθέτου ἕως δοθέντος· ὅτι συναμφότερος καὶ πρὸς ἣν ἥδε λόγον ἔχει δοθέντα λόγον ἔχει πρὸς ἀποτομήν· ὅτι ἐστὶ τὶ δοθὲν σημεῖον ἀφ' οὗ αἱ ἐπιζευγνύμεναι ἐπὶ [16]τόδε[q] δοθὲν περιέξουσι τῷ εἴδει τρίγωνον· ὅτι ἐστὶ [17]τὶ δοθὲν σημεῖον ἀφ' οὗ [18]αἱ ἐπιζευγνύμεναι ἐπὶ τόδε ἴσας ἀπολαμβάνουσι περιφερείας· ὅτι [19]ἥδε ἤτοι ἐν[r] παραθέσει ἔσται, ἢ μετά τινος εὐθείας ἐπὶ τὸ[s] δοθὲν νευούσης δοθεῖσαν περιέχει γωνίαν. ἔχει δὲ τὰ τρία βιβλία τῶν πορισμάτων λήμματα λ η, αὐτὰ δὲ θεωρημάτων ἐστὶν ρ ο α.

References to MS. No. 3.

[1] μέτἰ.

[2] ὡς.

[3] του not in MS.

[4] τῆσδε καὶ τὸ ὑπο δοθέντος καί τής δε ἴσόν.

[5] δοθέν τι.

[6] τῆς not in MS.

[7] ὡς.

[8] ἡ not in MS.

[9] λόγον.

[10] συναμφοτέρων.

[11] (συμαφοτέρου τῆσδέ τε καὶ) not in MS.

[12] λογου.

[13] ἀπο not in MS.

[14] υπο τωνδε οτι λογος του απο τησδε προς not in MS.

[15] τῆς not in MS.

[16] τὸ for τοδε.

[17] τι not in MS.

[18] αι not in MS.

[19] ἥδέντοι, but in the margin and in the same hand and ink with the MS. "ἴσως ἤτοι."

References to MS. No. 9.

[a] ως.

[b] τησδε και το υπο δοθεντος και τησδε ισον.

[c] δοθεντι.

[d] τῆς not in MS.

[e] ως.

[f] ἡ not in MS.

[g] λογον.

[h] αποτομην μετα δοθεντος λογον εχει προς αποτομην οτι λογον του υπο τωνδε προς αποτομην οτι λογος του.

[i] συναμφοτερων.

[j] λογου.

[k] απο not in MS.

[l] ολιγοι.

[m] τον ὑπὸ τῶνδε.

[n] λογον του απο τησδε προς το αποτομην.

N.B. There is against this line την in the margin written in the same hand and with the same ink as the text.

[o] δοθειση.

[p] τῆς not in MS.

[q] τόδε δοθὲν περιέξουσι τῶ εἴδει τρίγωνον· ὅτι ἐστί τὶ δοθὲν σημεῖον ἀφ' ου αἱ επιζευγνύμεναι ἐπὶ. not in MS.

[r] ηδεντοι.

[s] το not in MS.

I had intended to add (have, indeed, actually written them) a few remarks which are suggested by the letter of Professor Rigaud; but as they relate neither to Pappus, to Halley, nor to the Porisms, I shall reserve them till the occurrence in this series of a more miscellaneous set of notices. I may add, in conclusion, that the next of this series will complete the Nourse papers furnished me by Mr. Maynard. I only now interpose this paper on Pappus, as I wish to read the proof-sheets with the original before I return it to Mr. Noble.

Shooter's Hill,
March 8, 1850.

XLIX. *Notices respecting New Books.*

Hurricane Guide. By William Radcliff Birt. Murray.

THIS little work is devoted to the phænomena of revolving storms, and some few directions are given to seamen to avoid their centres. There are also some remarks upon atmospheric waves, and directions for taking meteorological observations.

The work is divided into five chapters. The first and fifth are devoted to revolving storms, and have been principally deduced from Colonel Reid's Theory of Storms. The second chapter is devoted to an explanation of Professor Dove's atmospheric waves, concluding with an urgent request to captains and masters of ships to make regular observations of the barometer, and the direction of the wind, both during the day and night. As the science of meteorology to seamen is of paramount importance, it is to be hoped that Mr. Birt's wishes will be complied with.

Chapter 3 is divided into five sections. The first treats of instruments, of observations, and of their reduction. The second, of the times of observation; but no mention is here made of the necessity of almost continuous observations during storms and gales of wind. (See Colonel Reid's book On Storms, 2nd edit. p. 418.) This section concludes with a repetition of the wish for the continuance of hourly meteorological observations on the days of the equinoxes and solstices.

These observations were first begun to be taken in the year 1833, and for a time they were sent to Professor Quetelet at Brussels; after this they were sent to Lamont at Munich; but at present it does not seem that there is any place for their reception. We know that observations of this kind have been sent to Munich; and neither has their receipt been acknowledged, nor has any use been made of them. If the numerous sets of these observations which have been made were collected, reduced and discussed, their results would be instructive, and possibly would suggest a different investigation. Till this is done we can scarcely join in the wish for their continuance. Nothing is more injurious to researches of this kind, than to urge observers to make observations, and then not to use them. In

Mr. Birt's book no mention is made of a depository for the observations he wishes made; but possibly he is willing to receive and reduce them himself, and to publish their results. Some such guarantee as this is needed, or we cannot expect gentlemen to engage in long and tedious series of observations.

The fourth section speaks of periods of extra observations, and particularly of that of the great atmospheric wave of November. Our experience does not confirm the existence of this particular wave. It seems to us that waves as regular and as large take place in the preceding and following months; and therefore, that if this period be made one of special investigation, it ought to be extended so as to include the months of October and December.

There is an appendix to the work, being a reprint of Tables I. and II. from the Report of the Committee of Physics of the Royal Society for the reduction of the readings of those barometers whose scales are of brass, extending from the centre to the top of the mercurial column.

The discussion of simultaneous observations of the barometer, and directions of the wind, have already led to unexpected rules to guide the mariner, to whose notice we recommend this work.

L. *Proceedings of Learned Societies.*

ROYAL IRISH ACADEMY.

AT the last meeting of the Royal Irish Academy, the Rev. Dr. Lloyd, D.D., the President, made the following observations on the recent destructive storm in Dublin:—

"Having watched attentively the progress of the late storm, and collected some facts relative to it from the records of the observatory, and from other sources, I avail myself of the present opportunity to lay them before the Academy. The phænomena were of a nature so unusual (I may say unexampled) in these climates, that it is desirable that some notice of them, however imperfect, should be placed on record, and the present summary of facts is offered chiefly in the hope that it may serve as a nucleus to a more complete one. I shall, of course, limit myself to those which have an immediate scientific bearing.

"From the tracings of the self-registering anemometer erected in Trinity College, it appears that on the 17th, and during the morning of the 18th, the wind blew gently from the S.W. Towards noon, on the latter day, it gradually veered to the S., and continued at that point until the arrival of the storm. This veering of the wind, however, appears to have been confined to the lower current. The direction of the upper current, as estimated by the motion of the clouds, was nearly S.W.

"The first indications of the approach of the storm were observed soon after three o'clock. Massive *cumuli* were seen forming in the south-western portion of the sky. These became denser as they approached, until they formed a mass of an ash-gray colour, pro-

jected on a sky of a paler tint, while the rugged outliers from the mass, of the peculiar form (between *cirrus* and *cumulus*) which indicates a high degree of electrical tension, showed plainly that a storm was approaching. About half-past three o'clock it burst forth. The flashes of lightning (generally forked) succeeded one another with rapidity, and at length the roar of the thunder seemed continuous. Some persons who observed the phænomenon from a distance were able to distinguish the two strata of oppositely electrical clouds, and to see the electrical discharges passing between them.

"Hitherto the wind was light, and there was that peculiar closeness in the air which is the result of high temperature and excessive humidity. Shortly before four o'clock the rain commenced—this was followed almost immediately by discharges of hail, and at four P.M. the terrific tornado, which was the grand and peculiar feature of this storm, reached us.

"This gale, which appears to have been a true whirlwind, first sprang up from the S.E., driving the hail before it impetuously. It then suddenly, and apparently in an instant, shifted to the point of the compass diametrically opposite, and blew with increased violence from the N.W. The noise about this time of the shifting of the wind was terrific, and arose (as is conjectured respecting similar tropical phænomena) from the confused conflict of hail in the air. The size of the hailstones, as well as the vehemence of the gale, appeared to be greater during the second phase of the storm than in the first. These masses, many of which were as large as a pigeon's egg, were formed of a nucleus of snow or sleet, surrounded by transparent ice, and this again was succeeded by an opake white layer, followed by a second coating of ice; in some of them I counted five alternations.

"In less than ten minutes the tornado had passed. The wind returned to a gentle breeze from the S.W., and the weather became beautiful. All the phænomena—the direction of the gale perpendicular to that in which the storm cloud was advancing, and the sudden reversal of that direction—seem to prove that it was a true tornado, whose centre passed directly over the place of observation. It is evident, on comparing the direction of the wind when the whirl first reached this part of the town with that of the progressive motion of the vortex itself, that its rotatory motion was retrograde, or in an opposite direction to that of the hands of a watch. It is deserving of notice also that this is the invariable direction in the northern hemisphere of the cyclones, or great revolving storms, to which the attention of meteorologists has been directed by Colonel Reid and Mr. Redfield. The late storm was, however, different from a cyclone, both in the dimensions of the vortex and in the causes from which it originated. The horizontal section of the cyclone where it meets the earth is often 500 miles in diameter; and the vortex is supposed to be the effect of two crossing currents of air, which generate a movement of rotation. In the tornado, to which species the late storm belonged, the vortex is of much smaller dimensions,

and is produced by rapidly ascending currents of air, caused by the heating of a limited portion of the earth's surface under the action of the sun's rays. In the temperate zones, accordingly, it is never produced in winter. These ascending currents are loaded with vapour, which (owing to the rapid evaporation) is in a highly electrical state, and when they reach the colder regions of the atmosphere the vapour is condensed, and electrical clouds are rapidly formed.

"The evidence relating to the direction of the gale, and its changes, as it passed over the College Park, is very complete and satisfactory. In the park and garden adjoining, nineteen trees were rooted up and prostrated, eleven of them being trees of large size. Of these ten have fallen from the S.E., or under the action of the first half of the gale, and nine from the N.W. Their bearings have been accurately taken, and the general result is, that the main direction of the S.E. gale, as indicated by that of the trees, is S. 56° E., and that of the N.W. gale N. 53° W. I believe that these results are even more accurate than those furnished by the anemometer; and they prove that in this locality the direction of the wind was exactly reversed, and therefore the centre of the vortex passed over the College.

"A remarkable circumstance connected with the direction of the fallen trees is their great uniformity, the individual direction seldom differing more than ten degrees from the mean. This is an indirect evidence of the great violence of the gale; and it proves, moreover, that the transition from the S.E. to the N.W. wind was immediate. There is greater regularity in the direction of the trees fallen from the N.W., than in those which have been blown down from the opposite quarter. This may have arisen partly from the greater violence of the gale in the former direction; but it is partly also due to the circumstance that the trees which fell from the N.W. are generally larger and in a less enclosed portion of the ground. It may be mentioned also, that the trees which fell from the N.W. generally lie to the southward of the others, as if there had been a shifting of the whole vortex in that direction. There are, however, two large trees in the garden lying side by side, but in directions diametrically opposed.

"It has been already stated, that in the College Park the shifting of the wind amounted to 180 degrees; and it has been inferred that the centre of the vortex passed over that spot. From what has been said as to the nature of the phænomenon, it will follow that in other localities, over which the vortex did not pass centrally, the wind must have shifted through different points of the compass, and through angles smaller in proportion to the distance from the centre. Thus, on the southern or south-eastern side of the line described by the centre of the vortex, the change of the wind should be from S. to W., and on the northern side of the same line from E. to N. We are not yet in possession of facts which bear upon this point; but from the limited dimensions of the vortex, and the consequent smallness of the distance necessary to produce such a variation, it is probable that evidence bearing upon it may readily be obtained. I shall only observe, that in seeking and comparing such evidence care must be

taken not to confound eddies arising from local obstructions with the general direction of the current.

"The observations of the barometer and of the dry- and wet-bulb thermometers made at the magnetical observatory on the day of the storm are the following:—

Hour.	Barometer.	Dry thermometer.	Wet thermometer.
7 a.m.	29·944	49·5	47·4
10	29·950	54·7	50·5
1 p m.	29·964	58·6	52·0
4	29·930	56·0	52·3
7	29·944	52·6	52·0
10	29·936	51·0	49·6

"The fall of rain and melted hail during the storm amounted only to 0·596 of an inch; but it is probable that the hail was driven out of the receiver of the gauge by the wind.

"It will be seen that the barometric fluctuation is small. It is stated, however, on what seems good authority, that a sudden and considerable fall of the barometer took place shortly before the storm.

"I have collected from the newspapers and other sources such information as I could obtain respecting the area of the city visited by the gale, but it is as yet very incomplete. It appears, however, that the diameter of the vortex was not very different from the length of the city from north to south; the limits of the gale being, namely, the Royal Canal on the north side, and the Grand Canal on the south. Hail fell, however, abundantly beyond the limits of the gale. Thus, at the gardens of the Royal Dublin Society at Glasnevin, the damage done by the hail was very great; but it was limited to the roofs of the houses, the hail having fallen perpendicularly.

"Further information is wanting also to enable us to determine exactly the progressive movement of the entire of the vortex. We are informed by the newspapers that a similar storm to that which visited Dublin, although not so severe, took place at Mullingar, about an hour and a half previously. If this be the same storm, the direction of its path must have been curvilinear. There seemed to be a disturbance of electrical equilibrium, accompanied by rain, in many remote parts of Ireland on the same day."

The President exhibited drawings of some of the hailstones which fell during the storm, and stated that they consisted of alternate layers of snow and ice, with a central nucleus of snow.

LI. *Intelligence and Miscellaneous Articles.*

ON OZONE. BY M. SCHÖNBEIN.

At the request of M. Becquerel, M. Schönbein repeated before him various experiments relative to ozone; of these the author furnished M. Becquerel with a detailed account, from which the following statement is extracted:—

M. Schönbein procures a large quantity of ozonized air by putting into a receiver of the capacity of 10 to 15 litres, a small quantity of water, and sticks of phosphorus of 1 centimetre in diameter, one-half of them being immersed in the water, and the other exposed to the air; the air of the receiver is heated from 59° to 68° Fahr. and imperfectly closed. When the operation is finished, which is ascertained by the odour of ozonized air, the receiver is inverted in a vessel of water, to get rid of the sticks of phosphorus, it is then removed and shaken in order to wash the compound. This operation being finished, a cork is fitted to the receiver into which two tubes are passed, one serving for the conveyance of water, and the other to conduct the ozonized air into vessels or tubes containing the substances to be submitted to its action.

Under the circumstances presently to be described, ozone is formed when the vapour of phosphorus and water are mixed with oxygen gas. It does not form at the same temperature in pure and dry oxygen: it is the same in pure and humid oxygen at the usual atmospheric temperature and pressure. Phosphorus, placed in moist oxygen, produces ozone at common temperatures, if the air is sufficiently rarefied.

Phosphorus, in humid oxygen, at common pressures, gives rise to ozone when the temperature is raised to 75° Fahr.; and its formation takes place rapidly at 86° Fahr. The presence of certain gases in the oxygen produces the same effect as rarefaction or increase of temperature. Of all gases hydrogen is that which produces this effect in the highest degree; then follow nitrogen and carbonic acid.

In a mixture of four parts of hydrogen and one of oxygen, as moist as possible, the formation of ozone is so rapid at 60° to 68° Fahr., that on account of the action occasioned by this body on the phosphorus, this substance and the explosive mixture inflame. Ozone is not formed in moist air at 32° Fahr.; the formation begins to be perceptible at 42° to 46° Fahr.; at 60° to 68° Fahr. it is rapid and occurs without danger. No effect takes place at common temperatures when the air is compressed to a fifth or a sixth; to obtain any action under these circumstances the temperature must be raised. The presence of certain gases, such as olefiant gas and nitrous vapours in humid oxygen, when sufficiently rarefied, and also in air, obstructs the formation of ozone.

According to M. Schönbein, when the vapour of æther is slowly burnt in air or in oxygen, there is formed, among other products, a compound of ozone and olefiant gas.

When ozone is passed through a tube heated to 482° Fahr., it is entirely destroyed. And the same is the case at common temperatures with charcoal. Ozone has the odour of chlorine if it is concentrated. When it is mixed with air, it has the odour which is perceived on turning the plate of an electrical machine.

Air which is strongly impregnated with ozone impedes respiration and produces catarrhal affections; small animals are quickly killed in it; it is insoluble in water; it quickly destroys organic colouring matter, and also ligneous and albuminous substances, &c. It com-

bines chemically with chlorine, bromine and iodine when water is present, and chloric, bromic and iodic acids are formed. Strongly ozonized atmospheric air, when exposed to lime-water, produces appreciable quantities of nitrate of lime. It may be stated, generally, that nascent ozone, when in contact with nitrogen and a strong base, produces nitric acid; a small quantity of nitric acid is also formed during the slow combustion of phosphorus in the air. Ozone acts powerfully on most metals, causing them to assume their maximum of oxidizement; the action commences at 32°. It combines directly with olefiant gas without decomposition; it destroys sulphuretted and seleniuretted hydrogen, &c., and converts sulphurous and nitrous acids, &c. into sulphuric and nitric acids.

Ozone precipitates peroxide of lead from an alkaline solution of lead, or from the acetate. It rapidly decomposes all the salts of manganese, whether in the solid state or in that of solution, producing peroxide. Hence it results that a strip of dry paper impregnated with sulphate or chloride of manganese is a reagent for ozone; the paper becoming rapidly brown in an ozonized atmosphere. Another and very sensible reagent, which M. Schönbein prefers, is a strip of starched paper containing a very small quantity of iodide of potassium. A solution of yellow ferrocyanide of potassium is changed by ozone into red cyanide. A great number of metallic sulphurets are rapidly converted by this substance into sulphates; such are the sulphurets of iron, lead, copper and antimony.

According to M. Schönbein, ozone is the most powerful oxidizing agent in nature. As ozone is invariably formed in the air by the action of artificial electrical discharges, it should also be produced throughout the atmosphere, in which natural electrical discharges occur. Nothing is easier than to demonstrate the presence of ozone in the atmosphere, and the variations of the quantities produced, by means of the test papers described. In general the reaction is greater in winter than in summer. M. Schönbein has always observed, that during a fall of snow it is much greater than at any other time. An exposure of iodized and starched paper for two hours is sufficient to render it of a deep blue colour, whereas the same air, enclosed in a receiver, produces no effect.

It may be inquired whether the nitric acid which is formed by passing electric sparks through air, as first observed by Cavendish, and also that produced during storms, are due to the direct action of electricity on oxygen and nitrogen, or to that of ozone on nitrogen.

Such are the general properties of a substance the composition of which has hitherto escaped all methods of analysis, and which M. Marignac considers as a peculiar modification of oxygen, which increases its chemical affinities. M. Schönbein regards it as a compound, probably containing more oxygen than oxygenated water. But these are merely hypotheses which require the sanction of fresh experiments. Opinions as to the nature of this substance should not yet be pronounced.—*Comptes Rendus*, 14 Janvier, 1850.

THE FIRST IDEA OF THE ELECTRIC TELEGRAPH.

Whenever it happens to be in my power, I feel a desire to render to Cæsar what belongs to Cæsar; on which account I wish the learned world to know that my late excellent friend Dr. Odier was the first who had the idea of the Electric Telegraph. Here is what he wrote seventy-seven years ago, namely in the year 1773, to Mdlle B . . .

"I shall amuse you, perhaps, when I tell you that I have some experiments in my head for obtaining the power of enabling the English, French, or other inhabitants of Europe to enter into conversation with the Emperor of Mogul or of China, so that without any trouble they shall be able to communicate whatever they may wish, at the distance of four or five thousand leagues in less than half an hour! Will this satisfy you as to glory? Nothing, however, is more real. Whatever turn these experiments may take, they must certainly lead to some great discovery; but I have not the courage to make them this winter. That which gave me the idea, was a word dropt accidentally the other day at the table of Sir John Pringle, where I had the pleasure of dining with Franklin, Priestley, and other men of great genius."

At that time Odier was much occupied upon the study of electricity. Some days before, he had thus written to Mdlle B . . . . . . "Is it not astonishing that the movement of a morsel of straw attracted by a piece of amber should have given Franklin the sublime idea of the lightning-conductor? Franklin is the first to have found the secret of imprisoning the electric fluid in a bottle."

Professor MAUNOIR, Geneva.

From the Bibliothèque Universelle de Genève for February 1850.

ACTION OF NASCENT CHLORINE ON LACTIC ACID. BY M. STAEDELER.

When a mixture of lactic acid or a lactate, common salt, peroxide of manganese, and sulphuric acid is subjected to distillation, a liquid is obtained, which, when mixed with potash, deposits oleaginous drops possessing the odour of chloroform.

If the quantity of chlorine is insufficient to act upon the lactic acid, aldehyde is chiefly obtained. This happens on distilling a mixture of one part of lactate of iron, four parts of peroxide of manganese and common salt, and four parts of sulphuric acid diluted with an equal weight of water.

When, however, the proportions employed are one part of lactate of iron, ten parts of peroxide of manganese and common salt, and ten of sulphuric acid, aldehyde is obtained only at the commencement of the reaction. If the products subsequently obtained by distillation be rectified over chloride of calcium, a colourless liquid separates on standing, which resembles chloral. In fact chloral may be separated from it by simple distillation; the liquid which comes over forms, with a small quantity of water, crystals of hydrated chloral; its solution in water furnishes chloroform by the action of potash.—*Journ. de Pharm. et de Chim.*, Novembre 1849.

ON THE SEPARATION OF CERTAIN ACIDS OF THE SERIES $C^n H^n O^4$. BY M. J. LIEBIG.

When it is proposed to separate a mixture of butyric and valeric acids, even when the quantities are small, the following process, according to the author, may be employed:—

Saturate a portion of the mixed acids with potash or soda, add the remainder, and subject the whole to distillation.

Two cases happen. If the mixture of valeric acid is contained in larger quantity than requisite to saturate the whole of the alkali, the residue no longer contains butyric acid, but pure valeric acid.

If the quantity of valeric acid contained in the mixture is insufficient to saturate the alkali, the remainder contains, besides the whole of the valeric acid, a certain quantity of butyric acid; but the product of the distillation consists of pure butyric acid.

The quantity of alkali to be added to the mixture ought consequently to be calculated according to the quantity of valeric acid which it is supposed to contain. If the mixture contains 10 per cent., one-tenth of the acid should be saturated. If it be required to separate 10 per cent. of butyric acid from impure valeric acid, nine-tenths of the mixture must be saturated.

From what is now stated, it is easy to perceive that one operation is sufficient to obtain one of the acids in a pure state. Either the product of the distillation is pure butyric acid, and the residue contains a mixture of valeric acid and butyric acid, or the product of the distillation contains a mixture of butyric and valeric acids, and then the product is pure valeric acid.

In repeating the same operations on the residue or the product of the distillation performed, there is again obtained a certain portion of one of the acids in a pure state; and by continuing these saturations and distillations, a complete separation of the acids may be effected.

As the boiling-points of butyric and valeric acids differ, it might be supposed that soda, when combining with the less volatile acid, would prevent its volatilizing at the temperature at which the other boils. In fact, when in a mixture of valeric or butyric acids, the first of these acids is fixed, it is evident that the second may distil in a state of purity.

A mixture of valeric and acetic acids, or of butyric and acetic acids, behaves in a totally different manner. When such a mixture is partially saturated with potash and submitted to distillation, it is not the acetic acid which passes over, as might be supposed, but the two other acids, although the point of ebullition of acetic acid is lower by more than 50° than that of butyric acid, and by more than 70° than that of valeric acid; in this case an acid acetate is formed which is not decomposed by the two less volatile acids.

If valeric acid be added to a neutral solution of acetate of potash, the acid dissolves in large quantity; whereas it remains in the state of oily drops in a solution of acid acetate of potash, in which it does not appear to dissolve in larger quantity than in water.

When valeric acid is added in excess to a neutral solution of ace-

tate of potash, and the mixture is distilled, part of the valeric acid distils, while in the residue there is a mixture of acid acetate of potash and valerate of potash.

When a mixture of acid acetate of potash and valeric acid is distilled, this acid distils and the acid acetate of potash remains free from valeric acid; butyric acid acts similarly to valeric acid with acetate of potash.

It results from what has now been stated, that when butyric or valeric acid, containing acetic acid, is partially saturated with potash, one of two things happens: either the acetic acid remains entirely in the residue with a certain quantity of butyric acid, and then the acid which distils is free from acetic acid; or the residue is formed entirely of acetic acid, and then the product of the distillation still contains acetic acid, which may be separated from the butyric acid or from the valeric acid by a second similar treatment.—*Journ. de Pharm. et de Chim.*, Mars 1850.

ON THE COMPOSITION OF THE PRECIPITATE FORMED IN SOLUBLE CYANIDES BY SUBACETATE OF LEAD. BY M. E. ERLENMEYER.

When hydrocyanic acid is added to subacetate of lead, no precipitate is formed; ammonia is requisite to produce it, by which a pulverulent white precipitate is formed and readily deposited.

In order to analyse it, M. Erlenmeyer washed it with water free from carbonic acid, collected it on a filter, and dried it on watch-glasses under a receiver containing lime and sulphuric acid. During drying the precipitate gradually changes from white to yellowish-white, and continually evolves hydrocyanic acid. When dry the decomposition ceases; it consists, according to the author, of $2PbO, Pb\,Cy$.

Its composition is the same whether aqueous or alcoholic solutions of ammonia or potash be employed in preparing it. The author adds, that it appears probable that the white precipitate is at first cyanide of lead, which is gradually decomposed by water into hydrocyanic acid and oxide of lead; the decomposition would stop when the composition of the substance is that indicated by the above formula. —*Ibid.*

THEORY OF COMPLEMENTARY COLOURS. BY M. MAUMENÉ.

The author has described, in a letter to the Academy of Sciences, an experiment which is interesting as regards the demonstration of the theory of complementary colours.

It is well known that the combination of two complementary colours produces white; and this is usually shown in lectures by employing two glasses, one of a red and the other of a green colour, the tints of which, although of considerable intensity, entirely disappear during the simultaneous interposition of the glasses between the eye and the source of light. M. Maumené several years since

arrived at the same result by using coloured liquors, and especially by mixing a solution of cobalt with one of nickel, both perfectly pure, and so diluted that their colour is nearly of equal intensity. The rose-red colour of the cobalt is completely destroyed by the green of the nickel, even in concentrated solutions, and the mixed liquid remains colourless.—*Journ. de Pharm. et de Chim.*, Mars 1850.

ANALYSIS OF THE WATERS OF THE MEDITERRANEAN. BY M. UZIGLIO.

M. Marcel de Serres has made a report of the different memoirs by M. Uziglio, relating to the analysis of the water of the Mediterranean.

A knowledge of the composition of the water of the Ocean and of inland seas, is highly interesting in a geological point of view, on account of the importance of these great fluid masses in the history of the globe. It is not less interesting to the chemist and the manufacturer, who works upon the salts which these waters contain. M. Uziglio rightly concluded that it was necessary again to analyse the water of the Mediterranean, the chemists who preceded him not having estimated with sufficient correctness the proportions of potash and soda which are held in solution.

The composition of the water of the Mediterranean cannot be compared to that of the Ocean, since it is circumscribed in a basin which is closed and limited, and hence it is more concentrated. In fact, the saltness of the seas appears to be maintained by the salts supplied from the water of the continents, and by the soluble substances which mineral waters supply in their courses. Thus the water is generally more salt near the coasts than in the open sea. On the other hand, mineral waters, and particularly salt springs, greatly resemble sea-water.

According to M. Uziglio, the principal substances contained in the Mediterranean are sulphuric, hydrochloric, hydrobromic and carbonic acids. MM. Figuier and Mialhe have also stated the presence of traces of phosphoric acid combined with magnesia. As to bases, M. Uziglio has observed potash, soda, lime, magnesia and oxide of iron, to which for the Ocean must be added oxide of manganese. The best-known and the most abundant element of sea-water is chlorine; in fact 100 grammes of the water of the Mediterranean contain 2·0468 grs. of it, and only 0·0432 gr. of bromine, which almost constantly accompanies the chlorine; both occur combined with sodium and potassium.

The most important point in the researches of M. Uziglio into the composition of the water of the Mediterranean, is the proof of the quantity of potash which it contains. According to his analysis, this quantity amounts to 0·0320 gr., or only 0·265 of potassium in 100 grammes. Notwithstanding the smallness of this quantity, M. Uziglio presumes that before long the potash extracted from the Ocean or the Mediterranean will replace the product of the lixivia-

tion of wood-ashes, just as soda artificially extracted from sea-salt has been long and advantageously substituted for that obtained from marine plants.

When the petrifaction of shells which occurs at present in the Ocean is considered, it excites no surprise that the proportion of lime is double that of the potash. In fact, 100 grammes of the water of the Mediterranean contain 0·623 gr. of lime, and the proportion contained in the Ocean, according to Figuier and Mialhe, is still larger. The carbonate exists in the Mediterranean in sufficient quantity to form considerable masses of shell-limestone analogous to those of the tertiary formation, and to be substituted for that which composed the shells in their fresh state. This new calcareous matter produces also true petrifactions, analogous to those of the geological æra.

One hundred grammes of the water of the Mediterranean contain 2·914 grs. of chloride of sodium, that is to say, nearly three hundredths; the next most abundant salt is the chloride of magnesium, of which 100 grammes contain 0·3219 gr.; whilst the sulphate of magnesia amounts to 0·2477 gr., and the sulphate of lime to 0·1357 gr.

The large quantity of sulphate of lime which the concentration of the water of the Mediterranean precipitates in the salt marshes would induce the belief that this salt existed in larger quantity; and if analysis does not show a larger proportion, it must not be forgotten that the mother-waters of the salt-works are frequently renewed. On this account it will be readily conceived, that after a certain time this salt may be deposited to a considerable extent.

Vegetables and animals contain considerable proportions of iodine, and yet the latest analyses do not indicate its existence either in the Ocean or the Mediterranean. It cannot, however, be inferred that these beings have the power of forming it; it follows only that the absorbent organs of vegetables and animals are more delicate and perfect than our best methods of analysis. The quantity of bromine found in sea-water prevents the detection of iodine; the production of the blue colour with starch may be effected or prevented, at pleasure, by repeatedly adding to a liquid an iodide or bromide: iodine cannot therefore be detected in sea-water while it contains bromine.

In a second memoir, M. Uziglio has examined the results of the evaporation of the water of the Mediterranean at different degrees of the areometer, and those of its analyses at different temperatures. He has given the result of his experiments on the deposits of salts comparatively with the thermometer and areometer; these tabulated results, however useful to the manufacturer, are not susceptible of analysis.

M. Uziglio has given a table (which is capable of being advantageously extended for the use of the manufacturer) of the different saline deposits obtained at different densities. The tables which precede it show that the progress of the continual evaporation of the water, in the salt-works, is identical till the density reaches 25°, and is pretty well maintained up to 30°; but beyond this, and when approaching 35°, the difference between day and night complicates

the phænomena, so that very variable mixtures of common salt, sulphate of magnesia, and chloride of magnesium are obtained.

The results of evaporation are still more variable above 35°. The mixtures of the salts deposited undergo numerous differences in their composition, without the possibility of foretelling the result of the precipitations: some contain from 0·5 to 0·17 of their weight of potash; it sometimes happens that this substance is found in deposits formed in solutions, the density of which is only from 34° to 35°; these deposits are derived from variations in the composition of the waters.—*L'Institut*, Fevrier 27, 1850.

ON NEW COMPOUNDS OF AMMONIA WITH FERROCYANIDE AND FERRIDCYANIDE OF NICKEL. BY M. A. REYNOSO.

When excess of ammonia is added to recently precipitated and moist ferrocyanide of nickel, it dissolves at first, changes its colour, and soon produces a precipitate composed of a multitude of very fine needles of a violet colour; these are the ammoniacal ferrocyanide of nickel. It may also be prepared by adding ferrocyanide of potassium to a solution of nickel containing much ammonia, or by causing a solution of the salt of nickel to act upon a mixture of ammonia and ferrocyanide of potassium. In every case the crystals of the salt are finer as they are more slowly formed; that is to say, when there is much ammonia, and the solution is consequently very dilute. The analysis of this salt indicated its formula to be $2NiCy, FeCy, 5NH^3, 4HO$.

When ferrocyanide of potassium is poured into a solution of ammoniacal nitrate of nickel, a greenish-white precipitate is obtained, which after being well dried is a mass of a very deep green colour, which becomes white by pulverization. It adheres to the tongue, is insipid, and completely insoluble in water. Ammonia dissolves it and converts it into quinto-ammoniacal ferrocyanide. Heat decomposes it, evolving ammonia and ferrocyanide of ammonia, leaving a carburet which fuses in burning.

This salt is the biammoniacal ferrocyanide of nickel; its formula is $2NiCy, FeCy, 2NH^3, HO$.

The ferridcyanide of potassium, poured into ammoniacal nitrate of nickel, produces a fine yellow precipitate, soluble in excess of ammonia. It is the biammoniacal ferridcyanide of nickel, the formula of which is $3NiCy, Fe^2Cy^3, 2NH^3, HO$.

All the ferrocyanides and ferridcyanides of the metals whose oxides are soluble in ammonia are themselves soluble in ammonia. The alkaline solution of ferridcyanide of cobalt is of a very deep red colour. The ferrocyanides and ferridcyanides of the metals whose oxides are soluble in potash, are themselves soluble in potash. Thus potash added to ferrocyanide of zinc produces immediately ferrocyanide of potassium and oxide of zinc, which dissolves in the excess of potash. If the potash be added with precaution, on filtering, the liquor contains merely ferrocyanide of potassium, and oxide of zinc remains on the filter. With ferrocyanide of mercury the reac-

tion is very distinct. This compound is white; on treating it with potash, there are produced ferrocyanide of potassium and yellow oxide of mercury, insoluble in excess of potash.—*L'Institut,* Avril 10, 1850.

EQUIVALENT OF CHROMIUM. BY M. LEFORT.

According to Berzelius, the equivalent of chromium is 351; other chemists make it 320 and 330: according to M. Lefort, the equivalent is 333.—*Ibid.*

METEOROLOGICAL OBSERVATIONS FOR MARCH 1850.

Chiswick.—March 1. Hazy: very fine: overcast. 2. Densely overcast. 3. Cloudy. 4. Cloudy: clear and frosty at night. 5. Frosty: fine: cloudy. 6, 7. Foggy: fine: clear. 8. Hazy. 9. Slight fog: fine: clear. 10. Very fine. 11. Clear and fine: frosty. 12. Frosty: very fine: clear. 13. Frosty, with slight fog: very fine. 14. Overcast. 15. Overcast: clear, with sharp frost at night. 16. Frosty: overcast: clear. 17. Frosty: cloudy: sunshine occasionally: clear and frosty. 18. Frosty and fine. 19, 20. Cloudy. 21. Cloudy and cold: clear and frosty. 22. Hoar-frost: cloudy. 23. Heavy clouds: sleet-showers. 24. Slight fall of snow-flakes: severe frost at night. 25. Clear and frosty: cloudy: clear: frosty. 26. Overcast. 27. Foggy: slight haze: frosty. 28. Slight haze: bright sun with dry cold air: clear and frosty. 29. Foggy: cloudy: clear. 30, 31. Overcast.

Mean temperature of the month	37°·71
Mean temperature of March 1849	41 ·56
Mean temperature of March for the last twenty-three years	42 ·81
Average amount of rain in March	1·36 inch.

Boston.—March 1, 2. Cloudy. 3. Fine: rain P.M. 4. Cloudy. 5—7. Fine. 8, 9. Cloudy. 10. Fine. 11. Cloudy. 12, 13. Fine. 14, 15. Cloudy. 16. Cloudy: rain A.M. 17, 18. Fine. 19. Cloudy: rain A.M. 20. Cloudy. 21. Fine. 22. Cloudy. 23. Fine: rain early A.M. 24. Cloudy: snow A.M. and P.M. 25. Cloudy. 26, 27. Fine. 28. Fine: rain A.M. 29, 30. Fine. 31. Cloudy: rain A.M.

Applegarth Manse, Dumfries-shire.—March 1. Fine: a shower early A.M. 2. Slight shower during night: thick P.M. 3. Rain heavy A.M.: cleared. 4. Frost: clear and fine. 5. Moist all day. 6. Mild and growing: moist. 7. Fine spring day. 8. Still finer: clear and warm. 9. Dull and moist: slight shower. 10. Clear and fine: slight shower. 11. Frost: clear and bright. 12. Frost: cloudy. 13. Frost, not so severe. 14, 15. Mild: cloudy: no frost. 16. East wind: dull. 17. Dull: raw: cold: shower. 18. Dull and cloudy, but mild. 19. Dull A.M.: cleared: rain P.M. 20. Very fine: slight drizzle. 21. Fine, though raw A.M. 22. Dry, but cloudy: stormy P.M. 23. Frost: snow: wind. 24. Hard frost: snow: calm P.M. 25. Frost very hard: thermometer $21\frac{1}{2}°$. 26. Frost slight: heavy snow A.M. 27. Frost severe: snow again. 28. Frost still harder: clear. 29. Frost still. 30. Frost moderate: hail: rain P.M. 31. Rain: cloudy and foggy P.M.

Mean temperature of the month	40°·3
Mean temperature of March 1849	41 ·8
Mean temperature of March for the last twenty-eight years.	39 ·6
Average rain in March for twenty-three years	2·35 inches.

Sandwick Manse, Orkney.—March 1. Showers: drops. 2. Drizzle. 3. Cloudy: showers. 4. Snow: cloudy. 5. Damp: rain. 6. Drizzle: cloudy. 7. Damp. 8. Fine: drizzle. 9. Cloudy: showers. 10. Sleet: aurora. 11. Bright: clear: aurora. 12. Drizzle. 13. Damp: drizzle. 14. Drizzle: damp. 15. Bright: damp. 16. Cloudy. 17. Drops: showers. 18. Hazy: cloudy. 19. Cloudy. 20. Bright: drops. 21. Cloudy: drops. 22. Showers: snow-showers. 23. Hail-showers: snow-showers. 24. Snow-drift: snow-showers. 25. Snow-showers: large halo. 26. Snow-showers. 27, 28. Bright: snow-showers. 29. Bright: clear. 30. Cloudy. 31. Drops: cloudy.

Meteorological Observations made by Mr. Thompson *at the Garden of the Horticultural Society at* Chiswick, *near London; by* Mr. Veall, *at* Boston; *by the* Rev. W. Dunbar, *at Applegarth Manse,* Dumfries-shire; *and by the* Rev. C. Clouston, *at Sandwick Manse,* Orkney.

Days of Month.	Barometer.							Thermometer.							Wind.				Rain.			
	Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.
1850. March.	Max.	Min.		9 a.m.	9 p.m.	9½ a.m.	8½ p.m.	Max.	Min.		Max.	Min.	9½ a.m.	8½ p.m.								
1.	30·332	30·209	29·86	29·92	30·00	29·68	29·82	53	38	41	51½	42	45	45½	sw.	wsw.	w.	w.	......	......	......	·03
2.	30·290	30·088	29·80	29·94	29·70	29·58	29·43	53	43	48	48	42	48½	48½	sw.	sw.	s.	sw.	......	......	......	·19
3.	29·878	29·661	29·37	29·40	29·48	29·34	29·63	54	33	46	45	42½	45	35	s.	ssw.	sw.	n.	·02	......	......	·20
4.	30·343	29·909	29·67	29·99	30·18	30·03	30·07	44	20	40	42	30½	32	36	n.	n.	nw.	ssw.	......	·08	......	·26
☾ 5.	30·536	30·526	30·06	30·20	30·28	29·89	29·95	48	26	35	50	35	47	47½	w.	wsw.	sw.	wsw.	......	......	......	·16
6.	30·621	30·547	30·10	30·30	30·33	29·96	30·12	56	27	43	51	37	48½	47	sw.	wnw.	sw.	w.	......	......	......	·13
7.	30·630	30·470	30·03	30·37	30·32	30·17	30·16	56	31	42	53	46	47½	48	sw.	w.	sw.	w.	......	......	......	·07
8.	30·433	30·322	30·00	30·26	30·19	30·25	30·23	47	38	44	55	40	47½	47	ne.	wsw.	nw-se	wsw.	......	......	0·25	......
9.	30·237	30·178	29·85	30·03	29·98	29·88	29·70	50	27	43	46	41	46	42	e.	ssw.	sw.	w.	......	......	......	·06
10.	30·258	30·243	29·85	30·03	30·20	29·74	30·15	54	28	40	50	38	40	39	nw.	nw.	w.	nw.	......	......	......	·20
11.	30·467	30·414	30·04	30·38	30·40	30·40	30·38	48	20	36	48	29	41½	43	ne.	nw.	nw.	w.	......	......	......	·07
12.	30·554	30·549	30·22	30·44	30·44	30·35	30·38	51	21	33	48	29	45½	45	n.	nw.	nw.	w.	......	......	......	......
● 13.	30·528	30·454	30·12	30·40	30·41	30·36	30·47	57	30	33	53	32	47	46	nw.	nw.	nw–w	nw.	......	......	......	·04
14.	30·475	30·458	30·06	30·42	30·41	30·51	30·51	47	34	45	52	41	43	44	ne.	n.	s.–e.	calm	......	......	......	·04
15.	30·449	30·399	30·07	30·40	30·32	30·46	30·40	47	18	41	52½	39½	46	43½	ne.	nne.	s.-sw.	nw.	......	......	......	·03
16.	30·275	30·142	29·88	30·25	30·22	30·36	30·32	48	23	41	49	41½	44½	42½	ne.	nnw.	ne.	sw.	......	......	0·00	·03
17.	30·303	30·301	30·00	30·20	30·21	30·23	30·24	48	17	33	40½	35½	45½	45	ne.	ne.	sw.	w.	......	·02	......	......
18.	30·286	30·197	29·87	30·20	30·13	30·23	30·20	44	24	33	48	31	47	46	ne.	e.	s.	wnw.	......	......	......	·02
19.	30·131	30·111	29·77	30·08	30·10	30·17	30·22	49	34	45	50½	41½	46½	44	n.	n.	n.	calm	......	·03	......	·01
20.	30·194	30·162	29·82	30·13	30·10	30·20	30·28	45	33	41	50	36	47	39	ne.	n.	se.	nne.	......	......	......	......
☽ 21.	30·224	30·162	29·77	30·19	30·20	30·29	30·14	44	19	41	49	36	43	43½	n.	n.	ne.	nw.	......	......	......	......
22.	30·214	29·784	29·75	29·98	29·59	29·78	29·59	49	30	38	48	37½	43	31	w.	w.	nw.	n.	......	......	......	·18
23.	29·529	29·504	29·20	29·48	29·51	29·45	29·64	44	27	34	47	28½	33	29	n.	nw.	nw.	n.	·01	·10	0·19	·12
24.	29·692	29·492	29·22	29·63	29·70	29·76	29·79	42	22	32	37	28½	29	29	nw.	nw.	n.	n.	·01	......	......	......
25.	29·769	29·736	29·45	29·68	29·54	29·38	29·70	42	14	29	40	21½	35	34	n.	nnw.	w.	e.	......	·23	......	·28
26.	29·791	29·767	29·50	29·59	29·70	29·77	29·83	42	19	23	40½	29½	37	35	sw.	w.	ne.	nw.	·01	......	......	·05
○ 27.	29·937	29·710	29·58	29·76	29·82	29·87	29·95	44	16	26·5	42	22	36	32	e.	w.	ne.	n.	......	......	......	·12
28.	30·106	30·042	29·70	29·92	29·95	30·00	30·03	49	16	27	43	20	37	31	w.	n.	se.	e.	......	......	......	·12
29.	30·104	30·008	29·82	29·92	29·85	30·01	29·99	49	29	31	48	26½	38	39	se.	ssw.	se.	se.	......	·04	......	·02
30.	29·935	29·744	29·68	29·73	29·56	29·86	29·68	52	37	41	42	35	41	39	se.	ese.	e.	se.	......	......	0·30	......
31.	29·751	29·737	29·40	29·43	29·50	29·54	29·58	55	33	41	50	36½	40	41	se.	s.	mod.	se.	·08	·09	......	......
Mean.	30·202	30·097	29·79	30·021	30·010	29·983	30·018	48·74	26·68	37·6	47·4	34·5	42·33	40·54					0·13	0·59	0·74	2·43

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LII. *On the Measurement of Temperatures by Thermo-electric Currents. By* M. Regnault*.

[With a Plate.]

It is known, from the beautiful discovery of Seebeck, that when a closed circuit is formed with two plates of different metals soldered at their extremities, and that the temperature of one of the junctions is raised, an electric current is formed, which is in general the more intense the greater the difference of temperature at the two places of soldering. Philosophers immediately sought to turn this property to account for measuring temperatures.

As we possess extremely delicate apparatus, by means of which, to a certain point, the weakest currents may be detected and measured, while on the other hand the metallic plates forming the circuit may be replaced by wires of very small diameter, exceedingly small thermoscopic apparatus, capable of demonstrating the feeblest variations of temperature, have been constructed. Every one is aware of the advantage which MM. Becquerel and Breschet have derived from these apparatus in measuring the differences of temperature exhibited by the several parts of the human body, and the beautiful results which Melloni obtained with his thermo-electric pile in his researches on radiant heat.

M. Pouillet has used the same principle for the measurement of high temperatures†; and he has described, under the name of magnetic pyrometer, an apparatus which is compared with his air pyrometer, and by means of which he states that he is able to measure the highest temperatures of our furnaces.

* Extracted from his *Recherches sur la Chaleur*. This is the memoir of which we promised to give a full translation in our former notice of M. Regnault's experiments, p. 56 of the present volume.—Ed.

† *Elémens de Physique*, 4th edit. vol. ii. p. 684. *Comptes Rendus*, vol. iii. p. 786.

The employment of thermo-electric couples for measuring temperatures would, under many circumstances, present such great advantages over the ordinary processes, especially when it is desired to determine temperatures within limited spaces, that I have frequently devoted attention to this subject; but I must confess, that, notwithstanding the very numerous and varied experiments which I have made, my researches have met with little success, and I have not been able to obtain a comparable instrument whose indications might at all times inspire confidence. There is such instability in the molecular conditions which determine the thermo-electric currents, that we are never certain of obtaining a current of constant intensity when the two solderings are raised at repeated intervals to the same temperatures; the variations are especially considerable when, during the interval, the apparatus has been heated to very different temperatures.

The instrument which is used for determining the intensity of currents is far from presenting the perfection which is necessary in accurate investigations, especially if these intensities vary considerably, as happens when thermo-electric currents are employed for the measurement of temperatures.

The intensity of electric currents is measured, either by the deflexions which they produce upon a freely suspended magnetic needle, or by the chemical decompositions which they effect. The second method, which is of great importance for the measurement of powerful currents, is inapplicable in the case of thermo-electric currents, which are always very feeble, and present too little after resistance to overcome the least obstacles introduced into the circuit.

The only instruments which have hitherto been employed for measuring thermo-electric currents, are based, therefore, on the deviations which these currents impress on the magnetic needle; they are the galvanometer and the sine compass.

The galvanometers with two partially compensated magnetic needles are most suited for measuring very weak currents; they appear, consequently, to be adapted principally for thermo-electric currents. Unfortunately, the deviations of the needles are not proportional to the intensity of the currents except between very restricted limits; and for somewhat considerable deflexions, it is necessary to construct a table in which the intensities corresponding to the observed deviations are found. The direct construction of this table would not be a matter of any moment if the same table were of service for any time; but experience has shown, that in a system of two partially compensated needles, the magnetic intensity

varies somewhat considerably, owing to circumstances which it is impossible to foresee and guard against, so that it is necessary to make this table very frequently. Frequently in the midst of a series of experiments a perceptible alteration may occur, and the experimenter is always uneasy on this point. Moreover, the sensitiveness of the galvanometer decreases rapidly with the amplitude of the deviations; and it must not be used to measure deviations of more than 60°, because beyond that limit the indications of the instrument become very uncertain.

A part of these inconveniences is obviated in the apparatus to which M. Pouillet has given the name of sine compass. In this instrument the needle is simple, and its magnetic axis is always placed so that it shall coincide with the direction of the current; the intensities of the current are then proportional to the sines of the angles which the magnetic meridian forms with the direction of the axis of the needle. To be convinced at any time of the identity of the measuring apparatus, it suffices to pass through the wire a current of constant intensity, and easily producible always of the same strength; if the needle indicates the same deflexion, we are certain that the apparatus has remained comparable.

The sine compass should not be employed to measure currents which produce deflexions of more than 50° to 60°; because beyond these limits the sines increase but very slowly for considerable variations of the arc, and the compass becomes useless. Thus all the measures on the sine compass should be comprised between 0° and 60°, and correspond to the intensities of the thermo-electric current between the limits of temperature which it is desired to measure. It thence results, that if we wish to determine elevated temperatures, it is requisite to be content with somewhat weak deviations for a difference of 100°, and the apparatus becomes not very sensitive. Thus, in the magnetic pyrometer of M. Pouillet, the compass indicated a deviation of 4° to 5° for a difference of temperature of 100° of the two solderings of the thermo-electric element. It is true, that, by giving a sufficiently large diameter to the divided circle, and observing the deviation with the assistance of a vernier, the subdivision of the degree may be carried as far as wished; and it is easy, if the divided circle has a diameter of 10 to 15 centimetres, to appreciate angles of 1′. The deviation produced by a difference of temperature of 100° would consequently be measured to within $\frac{1}{300}$; that is to say, with more than sufficient accuracy. Unhappily, the magnet needle is far from exhibiting any such sensitiveness. In the ordinary compasses the needle is supported on a pivot; and

with whatever care the bearing and the pivot have been worked, it is impossible to give to the needle a sufficiently great mobility to cause it to obey the feeble variations in the intensity of the current. It is necessary to give the instrument a few shakings to overcome the inertia of the needle; and the direction in which it stops, after having removed it from its position of equilibrium, varies very perceptibly, although the current always preserves the same intensity. Thus, in a perfectly constructed compass with an agate bearing, on which I have experimented, the uncertainty amounted to $\frac{1}{2}°$, which consequently gives an uncertainty of 10° per cent.

Much greater mobility is given to the needle by suspending it to a cocoon thread; but then other inconveniences arise, which occasion similar uncertainty. The accurate centring of the needle becomes difficult; it may perceptibly vary during the course of the experiments; the extreme mobility of the needle causes it to oscillate constantly around its position of equilibrium; it is difficult to alter the direction of the current, so that it coincide with that of the magnetic axis of the needle; and if the temperature which it is desired to measure is not absolutely stationary for a pretty long time, it becomes almost impossible to make an observation at the suitable moment. In all cases the measurement of the deviations presents great uncertainty, unless we have much time at our disposal to adjust the apparatus.

If the instrument is not to be employed to measure very high temperatures, but is merely to be used between 0° and 400° Cent., it may be arranged so as to obtain for a difference of temperature of 100° of the two solderings a deflexion greater than 5°. However, this is not always easy when it is desired to use but a single couple, and not to have recourse to a pile composed of several elements; when, moreover, this couple cannot be formed of those metals which produce the most powerful currents, as bismuth and antimony, on account of their great fusibility. Greater deflexions, it is true, are obtained by increasing the number of the convolutions of the wire which act upon the needle; but this increase has a limit, because the wires should possess great conductibility for thermo-electric currents, and consequently present a considerable diameter.

Any degree of sensibility may be obtained by substituting for the simple needle a system of two partially compensated needles; but then we meet with the same incoveniences as the galvanometer presents, especially those dependent on the magnetic alteration of the system. I have made numerous experiments with compasses arranged in this manner, but the

extreme mobility of the needles renders the manipulation very difficult.

The difficulties which are met with in the accurate measurement of the intensities of thermo-electric currents by the galvanometer and sine compass, induced me to seek for a process of measurement which was wholly independent of those instruments; and I think I have succeeded by the following method, which appears to me susceptible of successful application to the study of the laws of thermo-electric currents.

I constructed an element of bismuth and antimony composed of two bars, ABCD, Plate I. figs. 1 and 2, cast in the same mould. These two perfectly similar bars are juxtaposed throughout their extent and kept separate by a blade of ivory; they only touch at the two extremities, A and D, where the two solderings are. The length BC is 20 centimetres, the vertical pieces ABCD are 12 centimetres. This element of bismuth and antimony is the *normal element*, with which I compare all the other thermo-electric elements, but it should not be used for temperatures exceeding 30°.

The element destined for high temperatures is formed of an iron wire and a platinum wire of 1 millimetre in diameter; the extremities of these wires are soldered with silver. The iron wire, E*f*F, fig. 4, is about 80 centimetres in length; the two platinum wires, E*c*, F*d*, are fixed near to the iron wire, from which they are isolated by a non-conducting envelope. In their lower portion these wires are separated by a plate of thin glass. They are terminated by two brass appendages, *c* and *d*, which allow of the introduction of a galvanometric apparatus into the circuit.

The two junctions, E and F, are kept in glass tubes filled with a fixed oil containing no oxygen. One of these tubes is placed in the oil-bath mentioned in a former part of this work (p. 57), by the side of a mercurial thermometer extending from 0° to 350°. In some experiments an air-thermometer placed in the bath was employed. The tube which contained the second junction is kept at a constant temperature by means of melting ice, or placed in a great water-bath alongside the mercurial thermometer.

The normal element of bismuth and antimony, ABCD, figs. 5 and 6, is arranged in such a manner that the two solderings, A and D, dip into two vessels, MN, M′N′, filled with water at different temperatures, and separated from each other by a partition, SR. The same agitator, FGF′G′, serves to agitate at the same time the water in both vessels, and two very accurate and strictly comparable thermometers, T and T′, are placed near the two solderings. These thermometers

are the same as those used in my calorimetric experiments: each centigrade degree is divided into eighteen divisions, so that the differences of temperature of the two solderings can be measured with extreme accuracy.

Lastly, a very sensitive differential galvanometer completes the apparatus. This galvanometer has a system of two little compensated needles, to which is fixed a long hollow glass rod drawn out very fine. The extremity of this rod, which is blackened, moves on a divided quadrant 15 centimetres in diameter; it is observed by means of a telescope. The quadrant is divided into quarters of a degree; and it is very easy to appreciate deviations of $\frac{1}{8}$th, and even of $\frac{1}{10}$th of a degree. The sensitiveness of the galvanometer is such, that a difference of temperature of 1° in the two solderings of bismuth and antimony produces a deflexion of the needle of 17°.

The galvanometer is connected with the iron and platinum circuit by one of its wires, and with the bismuth and antimony circuit by its second wire.

With this arrangement, the junction of the iron and platinum, E, being maintained at a constant temperature t, if the junction F be raised to a temperature T' measured on the thermometer of the oil-bath, a current will result which will deflect the needle of the galvanometer; but by suitably raising the temperature of one of the solderings of the bismuth and antimony element, a second current, the inverse of the first, will be obtained, by means of which that may be neutralized and the needle of the galvanometer restored to 0. The temperatures θ and θ', which the two thermometers T and T' indicate at the moment of neutralization, are noted down.

Thus a difference of temperature, $T'-t$, between the two solderings of the iron and platinum, produces a current which is neutralized on the galvanometer by the current developed in the bismuth and antimony element by a difference of temperature $\theta'-\theta$. This difference of temperature, $\theta'-\theta$, is moreover much smaller than $T'-t$, because the electromotive force of the bismuth and antimony element is incomparably greater than that of the iron and platinum element, for with $T'-t=100$ we have $\theta'-\theta=6^{\circ}\cdot 5$.

If the oil-bath be raised to the temperature T'', θ' must be raised to θ'' in order to maintain the needle of the galvanometer at 0°. By continuing in the same manner, we obtain a series of temperatures $T'-t$, $T''-t$, $T'''-t$, &c., which will produce on the iron and platinum element currents which will hold *in equilibrio* on the galvanometer the currents produced on the bismuth and antimony element by the differences of temperature $\theta'-\theta$, $\theta''-\theta$, $\theta'''-\theta$, &c. If, consequently,

the two thermo-electric elements remain comparable, it will suffice once for all to form a table, in which are inscribed on one side the differences of temperature, $T'-t$, $T''-t$, $T'''-t$, of the iron and platinum element measured upon an air-thermometer, and on the other side the differences of temperature, $\theta'-\theta$, $\theta''-\theta$, $\theta'''-\theta$, of the bismuth and antimony element.

If it is now desired to measure a high temperature with the iron and platinum couple, it will suffice to look out the temperature, $\theta'-\theta$, which equilibrates it on the couple of bismuth and antimony; and we shall find in the table, the construction of which I have described, the temperature $T-t$ corresponding to it on the element of iron and platinum.

This method is entirely independent of the measuring apparatus. The magnetic state of the needle may vary without any inconvenience resulting, as it will be modified in the same manner for the two thermo-electric elements. The sole indispensable condition is, that the two elements should always remain perfectly comparable, and experiment will readily decide whether this condition is satisfied.

I shall not transcribe here the numerous experiments which I have made by this method, but shall content myself with relating in detail one of the series, and shall merely give the results of some others in order that the progress of the observations may be judged of.

First Series.

Iron and platinum element.		Bismuth and antimony element.		Differences of temperature.	
Soldering cold. t.	Soldering hot. T'.	Soldering cold. θ.	Soldering hot. θ'.	Iron and platinum. $(T'-t.)$	Bismuth and antimony. $(\theta'-\theta.)$
21·13°	100·10°	18·12°	23·06°	78·97°	4·94°
21·14	100·10	18·11	23·05	78·96	4·94
21·05	100·10	18·13	23·06	79·04	4·93
21·04	116·15	18·08	24·14	95·11	6·06
21·05	116·25	18·12	24·19	95·20	6·07
21·15	152·70	18·04	26·24	131·55	8·20
21·18	153·25	18·11	26·40	132·07	8·29
21·18	161·45	18·08	26·47	140·27	8·39
21·01	161·50	18·12	26·64	140·49	8·52
21·23	174·36	18·07	27·54	153·13	9·47
21·27	174·31	18·12	27·65	153·04	9·53
21·12	205·43	18·08	29·00	184·30	10·92
21·27	205·38	18·13	29·02	184·11	10·89
21·12	246·32	18·08	30·59	225·19	12·51
21·21	246·32	18·11	30·65	225·11	12·54
21·23	279·89	18·10	31·52	258·66	13·42
21·39	279·59	18·13	31·61	258·21	13·48
20·98	304·40	18·11	32·32	283·42	14·21
21·04	304·05	18·12	32·28	283·01	14·16
21·20	303·45	18·14	32·22	282·25	14·08

Second Series.		*Third Series.*		*Fourth Series.*	
Differences of temperature.		Differences of temperature.		Differences of temperature.	
Iron and platinum solderings. (T′−*t*.)	Bismuth and antimony solderings. (*θ*′−*θ*.)	Iron and platinum solderings. (T′−*t*.)	Bismuth and antimony solderings. (*θ*′−*θ*.)	Iron and platinum solderings. (T′−*t*.)	Bismuth and antimony solderings. (*θ*′−*θ*.)
22°·56	1°·60	46°·29	3°·18	84°33	5°·46
39·59	2·91	46·22	3·21	84·18	5·45
39·55	2·96	19·62	1·08	124·91	7·85
84·17	5·69	19·47	1·06	124·93	7·83
83·90	5·64	82·05	5·18	183·36	10·64
83·82	5·65	81·87	5·19	183·26	10·64
81 11	5·36	101·37	6·37	239·14	12·65
81·06	5·45	101·97	6·49	239·39	12·67
90·04	6·24	131·07	8·07	280·15	13·88
103·66	6 88	132·35	8·16	280·80	14·02
102·29	6 85	178·23	10·38		
127·30	8·99	177·89	10·42		
126·90	9·03	147·30	9·04		
153·79	10·56	147·47	9·04		
153·59	10·58	163·53	9·82		
174·48	11·70	163·78	9·87		
204·28	12·94	199·65	11·53		
204·07	12·87	199·98	11·65		
235·30	13·94	226·36	12·68		
234·97	13·86	225·89	12·71		
259·22	14·61	249·45	13·50		
259·37	14·55	250·05	13·61		
283·74	15·28	278·31	14·61		
282·40	15·13	278·36	14·61		

I have represented graphically the results of these experiments. To do this easily I took for ordinates the differences of temperature of the iron and platinum junctions divided by 3, and I took for abscissæ the differences of temperature of the bismuth and antimony junctions multiplied by 5.

The conditions being apparently identical in these several series of experiments, the corresponding curves ought to be susceptible of being superposed: but such is not the case; in some instances the curve exhibits a very satisfactory regularity throughout its extent; in other cases, on the contrary, and without its being possible to ascertain the cause, there is a sudden leap at a point, and the second portion of the curve no longer agrees with the first: very rarely the curves furnished by two series of experiments approach sufficiently for the differences to be attributed to errors of observation, and the two curves to be considered as the expression of the same phænomenon.

These variations are probably owing to changes which take place in the molecular condition of the metals at the place of the solderings, and which suffice to modify perceptibly the electromotive forces. Sometimes these changes come on sud-

denly in the midst of a series of experiments, they then produce the leaps which are observed in the curves; in other cases, on the contrary, the alterations are but slowly effected, and they are only detected by causing the elements to pass through the same temperatures.

I thought it might perhaps be possible to cause these irregularities to disappear by avoiding the small quantity of solder which joins the two metals*, and by giving a great section to the worst conducting element. For this purpose I constructed the element represented in fig. 3. A hollow iron tube was curved while red-hot in the direction ABCD; the upper portion of this tube was filed away so as to convert the portion BC into an open channel. The extremities A and D were hammered while red-hot in order to cause the interior aperture to disappear almost entirely, and in it was incorporated, at a white welding heat, two platinum wires of one millimetre in diameter. These wires are placed in the interior of the hollow tubes AB and CD; and to isolate them from the iron sides, they were covered with glass tubes; they terminate in two brass binding-screws, *a*, *b*, situated the one alongside the other, and by means of which the element is brought into connexion with the galvanometer.

This new thermo-electric element was arranged in the apparatus in the same manner as in the preceding experiments; the portion which dipped into the bath was kept in a tube full of oil. I will here give the results of some experiments.

Fifth Series. Differences of temperature.		*Sixth Series.* Differences of temperature.	
Iron and platinum solderings. $(T'-t.)$	Bismuth and antimony solderings. $(\theta'-\theta.)$	Iron and platinum solderings. $(T'-t.)$	Bismuth and antimony solderings. $(\theta'-\theta)$.
96·76°	6·24°	120·88°	9·21°
96·32	6·25	120·86	9·20
163·52	9·75	114·31	8·70
163·69	9·71	113·21	8·65
179·94	10·38	158·87	11·69
179·41	10·43	158·94	11·69
217·99	12·01	150·77	11·37
217·16	11·88	151·13	11·36
268·64	13·71	186·71	13·40
270·02	13·65	186·81	13·52
269·89	13·50	216·93	15·15
274·76	13·61	217·07	15·31
273·46	13·55	268·77	17·87
		268·66	17·77
		285·75	18·08
		285·72	18·03

* In the elements employed for the preceding experiments, the two wires of iron and platinum were soldered with silver, but the quantity of silver was inappreciable.

Seventh Series.

Differences of temperature.		Differences of temperature.	
Iron and platinum solderings. $(T'-t.)$	Bismuth and antimony solderings. $(\theta'-\theta.)$	Iron and platinum solderings. $(T'-t.)$	Bismuth and antimony solderings. $(\theta'-\theta.)$
$103^{\circ}\cdot80$	$8^{\circ}\cdot27$	$221^{\circ}\cdot60$	$15^{\circ}\cdot75$
103·40	8·23	282·18	18·41
117·92	9·08	281·46	18·51
117·96	9·30	149·77	12·36
117·96	9·29	148·97	12·30
152·19	11·64	195·67	15·01
152·29	11·69	195·31	14·97
189·69	13·99	268·76	18·76
188·91	14·00	268·56	18·60
221·95	15·72	268·06	18·55

I represented by the graphic method the results of these new experiments as I had done for those of the first. I ascertained thus that the three curves could not be superposed, but that nevertheless they exhibited less considerable deviations than those of the first four series.

In the second series the oil-bath was allowed to cool after the observation made at 281°, without disturbing any portion of the apparatus, and the experiments recommenced when the temperature of the bath had descended to 140°. It should be observed, that the portion of the curve corresponding to this second period does not agree with that which is given of the first, and nevertheless the apparatus had undergone no change.

I have made numerous experiments on the bismuth and antimony couple in order to ascertain whether these irregularities were not principally due to that couple; but by varying the temperature of the junctions between the limits which had been attained in the preceding experiments, viz. from 15° to 33°, I found that the bismuth and antimony element remained pretty constant. I observed that a difference of temperature of 1° between the two solderings produced sensibly the same deflexion of 17° on my galvanometer, whatever was their absolute temperature, provided it always remained comprised between the limits above indicated. But it is difficult to decide whether this proposition is exact, or merely approximate, because the intensity of the current varies perceptibly with the time, even when the two junctions constantly present the same difference of temperature; and there always remains a little uncertainty as to the value of the deflexion which should be inscribed.

But I found, contrary to the opinion generally admitted, that an increase of 1° in the difference of temperature of the

two junctions of the bismuth and antimony element developes an electromotive force, weaker in proportion as the difference of temperature is greater, even between the limits 15° and 35°.

This result is easily verified in the following manner. The bismuth and antimony element being arranged in the two vessels full of water, as in fig. 6, and in communication with the galvanometer, the needle is brought to zero by raising the water of the two vessels to exactly the same temperature; a certain quantity of hot water is then poured into one of the vessels, so as to produce precisely a difference of temperature of 1° between the two solderings. The deflexion n of the needle is noted down.

After this a very feeble and perfectly constant hydro-electric current is passed through the second wire of the differential galvanometer: the needle is deflected a certain quantity by this current, but it is brought back to zero by suitably raising the temperature of one of the bismuth and antimony junctions. The two currents then hold each other *in equilibrio*. The temperature of the same junction is raised 1°, whence results a deflexion of the needle, which is precisely equal to the deviation n previously observed, if the electromotive force developed by an increase of 1° in the difference of temperature is the same whatever that difference may be.

By passing in this manner successively stronger and stronger *constant* hydro-electric currents through the second wire of the galvanometer, and neutralizing them each time by a suitable difference of temperature between the two solderings of the thermo-electric current, I found that the electromotive force developed by an increase of 1° of difference of temperature was the more feeble as that difference was greater. The thermo-electric element formed of wires of iron and platinum is not the only one which I have experimented with at high temperatures; I likewise made some trials with elements composed of other metallic wires. But the element of iron and platinum has proved to be most suitable; it is that the electromotive force of which decreases least with the elevation of the temperature.

The sensitiveness of a couple of iron and copper decreases very rapidly with the temperature. At about 240° an elevation of 20° to 30° no longer exerts any influence upon the needle, which remains perfectly stationary: the needle retrogrades when the temperature is raised higher; and the intensity of the current, far from increasing with the temperature, now decreases. This observation agrees with what M. Becquerel long ago observed on a couple of iron and copper. According to that able philosopher, the current is even esta-

blished in a direction contrary to its primitive one when the element of copper and iron is heated in the flame of a spirit lamp*.

I have likewise on several occasions made experiments on thermo-electric currents by interposing variable resistances in the circuit, so as to maintain the needle of the galvanometer at a constant deflexion for the different temperatures communicated to the solderings. For that purpose, I employed sometimes Prof. Wheatstone's rheostat, sometimes a simple metallic wire strung by a weight, and of which I inserted different lengths in the circuit; but I thus obtained much more variable and uncertain results than by the method above described. I added in this manner to anomalies produced by the thermo-electric elements themselves those due to the irregularities of the conductibility of the resisting wires, which will always render this method very uncertain for the study of feeble electric currents.

To conclude, if the numerous experiments which I have made on the thermo-electric currents do not demonstrate that these currents cannot in future be employed for the measurement of temperatures, they at least show that we are still far from being acquainted with all the circumstances which exert influence on the phænomenon, and of being able to establish the conditions in which the thermo-electric elements ought to be placed so that the intensities of the currents may depend solely on the temperature.

LIII. *On the Hail Storm of May* 5, 1850, *as observed at the Kew Observatory. By* Mr. W. R. Birt†.

To Richard Taylor, Esq.

Kew Observatory, Old Deer Park,
Richmond, Surrey, May 14, 1850.

My dear Sir,

By permission of the Committee of the Kew Observatory of the British Association for the Advancement of Science, I annex a record of observations made at Kew on the Hail Storm of May 5, 1850, and beg to add my views of the phænomena.

"At half-past 5 in the afternoon, on May 5, 1850, my attention was arrested by a very heavy and dark collection of clouds (*cumuli*) in the N.E., from which rain was falling in the distance. The appearance of the clouds was very black and threatening. Below this black collection, which occupied a

* *Ann. de Chim. et de Phys.*, vol. xxxi. p. 385.

† Communicated by the Author.

very considerable portion of the sky from the zenith to the eastern horizon, the cloud, apparently a sheet of *cirrostratus*, presented a most remarkable reddish hue. Cirrostratus had been prevalent during the day. At 9 A.M. I registered *cirrus*, *cirrostratus*, *cirrocumulus* and *cumulus*; and at 3 P.M. *cirrostratus* and *cumulus*. On both occasions these clouds were moving from the *south-west*. At 3 P.M. I observed a very splendid solar halo; the portion of it not covered by cumuloid clouds was exceedingly well-defined, the angular distance between its upper portion and the sun being 21° 49′. It continued more or less visible until 4 P.M., and presented an elliptical form, the lower portion being much further from the sun than the upper. The interior boundary of the halo exhibited a reddish-brown tint, gradually passing towards the exterior boundary into a softened white, which thinned off into the general mass of cloud; the *darkness* of the interior space, especially near the halo, was very striking. When I first noticed the threatening aspect of the clouds, those in the immediate neighbourhood of the observatory were moving from the S.W. At 5^h 40^m P.M. a few large drops of rain fell; they were almost immediately succeeded by hail (small), and hail mingled with heavy rain continued to fall until 6^h 10^m. During this time the process of nimbification proceeded rapidly, *extending from the N.E. towards the S.W.; so that although just previous to the commencement of the storm the clouds were moving in the opposite direction, the action, now very considerable, rapidly converted the cumuli hovering over the observatory, and more probably to some distance S.W. of it, into nimbi, accompanied with a very copious precipitation of hail.* While this active precipitation of hail was proceeding, the electrical conductor became charged to about 40° of Henley's electrometer, the pendulum vibrating rapidly at the time when the hail came down in greatest abundance and of the largest size, and sparks were observed 0·3 inch in length.

"The heavy rain previously noticed ceased at 6^h 27^m, and during the 47 minutes of this portion of the storm the temperature declined 4°·7. After this a lighter rain fell until 7^h 10^m, in the course of which the tension, as manifested by the conductor, increased: the vibrations of the pendulum of Henley's electrometer also increased; on one occasion it reached 90°, with sparks 0·70 inch in length. During the storm the charge was generally negative."

We have in the above phænomena an illustration of Mr. Howard's remark (see vol. xxxvi. p. 167):—"This rain opens an immediate communication with the earth; the positive electricity, which before rendered the particles buoyant,

STREAMS DOWN ALONG WITH THE RAIN AND THROUGH IT; *and the shower is propagated in all directions till the whole mass of cloud is brought into action.*" The great body of cloud, as appears from observations at the commencement and close of the storm, was moving from the S.W., from which the legitimate inference is, that the storm itself was moving from S.W. to N.E. When the black threatening appearance was first noticed in the N.E., rain was falling from the bases of the cumuli; it did not, however, appear to be of great extent; and at the same time the clouds in the S.W., W. and N.W., presented, especially as contrasted with those in the N.E., a remarkable *light* appearance: *the storm did not pass over this observatory from the S.W.* Shortly after the rain commenced, and during the continuance of the hail, the well-defined black masses of cumuli were rapidly resolved into nimbi; and it appeared to the writer that this resolution of cumuli into nimbi *occasioned the apparent progression of the storm from the N.E.* Unfortunately he did not particularly observe the character of the clouds in the S.W., W. and N.W.; but his impression is, that during the entire period of the storm the atmosphere was much *lighter* in those directions. It would be extremely interesting to learn at what point nimbification began, and how far it extended on each side the nucleus. The apparent conclusion from the phænomena is, that when nimbification commenced it rapidly extended in *all directions* from a central point.

From information that I have received, I have some reason to conclude that the area over which the hail fell was very circumscribed. The hail was not noticed either at Mortlake or Petersham, both near Richmond; the clouds were moving towards the former village, and the dark threatening appearance was seen in that direction; so that the nucleus was most probably between Richmond and Mortlake, but nearest the former place. The storm was observed at Ham.

The phænomena above detailed appear to me so illustrative of the views embodied in my paper on the connexion of Atmospheric Electricity with the Condensation of Vapour (vol. xxxvi. p. 161), that I have thought they may not be altogether uninteresting to the readers of the Philosophical Magazine.

Will you be kind enough to notify in your next Number that I shall be happy to *receive* from any quarter observations made in conformity with my instructions in the Hurricane Guide? if corrected, so much the better; and as opportunity may offer, such observations shall undergo *discussion*, at least insofar as they may throw light on the great aërial move-

ments mentioned in the Guide, and the results be communicated to some scientific body.

With regard to the remarks on the November wave by the writer of the notice, I am quite of opinion that the movements I have observed are not confined by any means to November, nor do they extend *only* to the three autumnal months, but are to be detected in every month all the year round. Nor do I apprehend that these movements stand out more prominently in November than at any other time. The only reason that they have been brought more prominently into notice in November is, that more attention has been paid to the barometric phænomena in that month. The mode of inquiry is, however, one involving considerable labour, and requiring numerous stations; so that unless a gentleman is able to devote to it the whole of his time, or at least a very considerable portion of it, the progress in the general investigation must be but slow. Some of the most valuable data would be obtained from marine observations.

I have the honour to be, my dear Sir,

Yours very respectfully,

W. R. Birt.

LIV. *Eighth Memoir on Induction. By* M. Elie Wartmann, *Professor of Natural Philosophy in the Academy of Geneva**.

[Continued from vol. xxxiii. p. 446.]

§ XX. *On the use of the multiplying Rheometer for measuring differences of intensity of weak or powerful Electric Currents.*

211. THE measure of the variations of intensity of galvanic currents is ordinarily effected by the aid of the voltameter. But the employment of this apparatus requires various precautions, without which we arrive at false results, and diminishes the energy of the current by the whole of the resistance proceeding from the liquid under decomposition. It is often more convenient to employ the galvanometer, which does not expose us to the same errors. I do not speak of the coarse multipliers with a single very heavy needle, such as were constructed twenty years ago, but of rheometers with a longer or shorter wire, and furnished with an almost astatic system of light needles delicately suspended.

212. If the current which has to be appreciated is sufficiently weak not to heat a thin copper conductor, it is distributed between the two equal wires of a differential galvanometer, giving it an opposite direction in each of them, or the

* From the *Bibliothèque Universelle de Genève*, January 1850.

differential resistance-measurer of Professor Wheatstone is employed. The needle remains at zero under the influence of the two contrary actions which affect it. It is only necessary then to place a rheostat in the circuit of one of the wires to diminish its conductibility, and give to the current circulating in the other an increasing preponderance. When the index has been brought to the fortieth or fiftieth degree, it detects the feeblest variations in the intensity of the current.

213. This method is inapplicable when the current is very powerful. A part of its intensity would be spent in overcoming the resistance of the wires of the instrument, and the latter might be deteriorated. In this case we must not circulate the whole of the current through the rheometer, but only introduce into it a part, the amount being in proportion to the delicacy of the apparatus.

214. This may be effected in two ways. Let us suppose that the wires of the galvanometer terminate in two glasses filled with mercury, in which are plunged the extremities of the circuit traversed by the voltaic current. A conductor will serve to unite these two glasses. This conductor may be a rheostat, which, by changing the length of the communicating wire, will determine a variation in an inverse direction in the intensity of the part of the current which is conveyed to the multiplier.

215. We may also give constant dimensions to the conductor which joins the two glasses, and connect this rheostat with the rheometer. It is always possible to separate in this manner a part of the whole current proportioned to the sensitiveness of the instrument without weakening the energy of the action of the battery upon the substances comprised in its circuit. The measure of the variations of this energy will be diminished in the relation of the abstracted to the total current; but this diminution will be compensated by the perfection of the system of needles subjected to the influence of the abstracted current, as long as the battery is not of very great power*.

§ XXI. *Does Electro-magnetic Induction modify the conductibility of bodies for Electricity?*

216. Philosophers have carefully determined the electrodynamic relations which are established between a magnet and a conductor traversed by an electric current when one of

* This method of derivation has already been employed in my second memoir (74.). See the important work of Mr. Wheatstone, entitled "An Account of several new Instruments," &c., Phil. Trans. 1843, part 2, p. 322; or *Ann. de Ch. et de Ph.*, vol. x. p. 288. Third Series.

these bodies is moveable. They have studied the remarkable effects which the magnet produces upon the voltaic arc, and the sounds which it gives rise to in the conductors traversed by discontinuous currents. But they appear not to have investigated the influence which might be exercised by the magnetic or diamagnetic state developed in various substances on their conducting power. The following experiments are intended to fill up this blank.

217. An electro-magnet was set in action by a Grove's battery of ten large pairs, and rendered capable of raising several hundredweight. The conductors of the battery communicated with two little cups filled with mercury. The wires of the electro-magnet terminated in two other similar cups. In effecting the junctions by the aid of the commutator already described (152.), the direction of the magnetization was made to vary at will, and very rapidly.

218. A zinc and copper pair, of small dimensions, was plunged into distilled water. The current was conveyed by long strips of thick copper, intended to comprise in their circuit the bodies to be magnetized, and an excellent Ruhmkorff's rheometer far enough off to experience no perturbation.

219. The first conductor submitted to trial was the soft-iron armature of the electro-magnet, isolated from the poles by the interposition of a thin sheet of paper. Whatever were the energy of the magnetism produced, and its direction relatively to the direction of the current which traversed the armature, the index of the rheometer remained in one constant position.

220. Being desirous of giving greater certitude to this result, I employed a soft iron wire, perfectly annealed, and twisted fifty-eight times upon itself. The whole length of this wire was two metres, and the length of the convolutions equal to the distance of the poles from the magnet. After isolating all the folds with very dry paper, I substituted this wire, three millimetres thick, for the armature of the previous experiment. The multiplier was not affected by the production of sixty contrary polarities conferred consecutively and at equal intervals during the passage of the voltaic current.

221. A solution of sulphate of protoxide of iron was disposed axially on the poles. Its conductibility was not modified by the magnetization.

222. I then replaced the iron and its solution by diamagnetic metals. Some bars of bismuth and others of antimony were first tried in the axial position. They were then placed equatorially, after covering them with a thin piece of paper to isolate them from two soft-iron armatures, of the same length

as themselves, and each resting on one of the poles of the magnet. The induction was again without influence.

223. The experiment was repeated with a bar of bismuth-antimony. Two polar armatures concentrated the maximum of magnetic energy upon the solder, when the cylinder was perpendicular to the axis of magnetization. This heterogeneous conductor was traversed by the current of the pair in directions successively opposite; the position of the magnetic poles was also changed. In spite of these alternatives, the absolute value of the deviation of the galvanometer remained invariable.

224. I wished to anticipate the objection that the electric current passed through my different conductors was too weak not to be admitted without modification, whatever were the molecular changes caused by an intense magnetism, and those which depended on the cessation of that force. I therefore replaced the small cell (218.) by three Daniell's cells of large dimensions, charged with some concentrated sulphate of copper and strongly acidulated water. For the Ruhmkorff's rheometer I substituted one of Gourjon, No. 27, the wire of which is much longer. A rheostat was connected with this instrument, and the derivation of the rheometer current was effected as has been stated (215).

225. The repetition of all the preceding experiments led to the same conclusion as before.

226. A bismuth-iron cylinder was placed first axially, then equatorially. The direction of the magnetization was successively deranged. Some armatures magnetized the solder in the diamagnetic position with extreme energy. The result did not vary.

227. Lastly, I tried the experiment on different liquids, as the chlorides of nickel and cobalt dissolved in distilled water, and on acid solutions of nitrate of bismuth and chloride of antimony. These solutions were contained in glass tubes closed by stoppers which admitted the passage of metallic wires. Here again the rheometric deviation was not at all affected by the induction of the magnet.

228. It remained to examine the case of a current still more intense, traversing the magnetized conductor. I employed that of a battery of ten large Daniell's elements. But in order not to take measures on too small a portion of the active electricity, the multiplier was replaced by a voltameter with very large plates of platina.

229. The armature of the electro-magnet, placed in the circuit, did not vary in conductibility, in whatever direction it was magnetized or brought to its ordinary state.

230. The long iron wire (220.) behaved as before.

231. Lastly, I examined under the same point of view some bodies having an atomic rotatory power on the polarized waves of light and heat. I applied to them the principle of derivation mentioned (214.), employing Gourjon's rheometer, and a voltaic intensity proportioned to their conducting power.

232. Some sugar syrup in a glass tube was placed between the poles of the electro-magnet. The current of ten Daniell's pairs was made to pass through it a length of some millimetres. The multiplier was not affected by the magnetization, whatever was its direction.

233. A concentrated solution of sulphate of quinine in distilled water, traversed by a current of five pairs, gave the same result.

234. Concentrated tartaric acid, submitted to the current of a single Daniell's pair a length of three centimetres, was not modified by the magnetic induction.

235. It follows from these researches that magnetization does not alter the molecular condition developed by the passage of an electric current so as to affect its conductibility*. The inverse proposition would also be verified in all probability. If, then, electricity results, as some physicists suppose, in ætherial movements depending on the surrounding matter, these movements preserve their intensity when this matter is acted upon by the forces which emanate from the poles of an energetic magnet. This circumstance must be taken into account by the theories which pretend to explain the phænomena of electricity and magnetism.

December 24, 1849.

LV. *On the Geometrical Laws of the Motion of a Rigid System about a Fixed Point.* By W. F. Donkin, *M.A., F.R.S., F.R.A.S., Savilian Professor of Astronomy in the University of Oxford*†.

THERE is a very simple theorem which seems to be capable of useful applications in the theory of the motion of a rigid system. I do not remember ever to have seen it explicitly stated, though Mr. Boole appears to refer to it in a paper published some time ago in this Journal, which will be mentioned below. I propose here to give a geometrical de-

* After digesting these notes, I have found in the *Traité de Physique* of M. Péclet an isolated observation which agrees with the facts examined above, although the author has not drawn from them any conclusion relative to the nullity of the influence of the magnetism upon the conductibility. (See vol. ii. p. 265, No. 1142; 4th edition, 1847.)

† Communicated by the Author.

monstration of it, after establishing the necessary definitions and conventions.

In considering the motion of a rigid system about a fixed point, let us conceive a sphere to be described about that point as a centre, and fixed in space; and another sphere, with the same centre and radius, to be invariably connected with and carried by the moveable rigid system; so that the motion of this latter sphere may be substituted for that of the rigid system, so far as its geometrical displacement only is concerned. For convenience, let the radius of each sphere be taken equal to the linear unit.

When a rotation takes place about any axis passing through the centre, let the points of intersection of that axis with either sphere be called the poles of the rotation; and let that be called the *positive* or *north* pole, with respect to which the rotation has the same direction as the earth's diurnal motion with respect to its north pole.

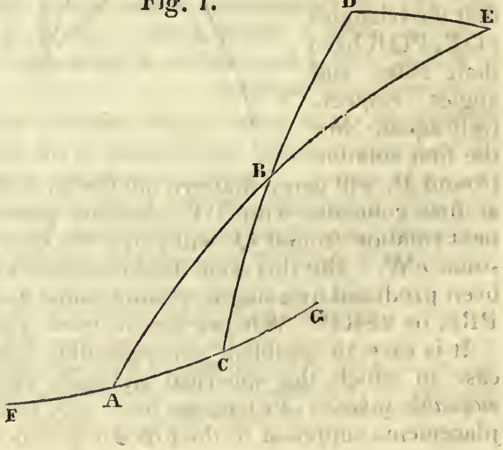
Suppose A, B are any two points on either sphere; we may use the abbreviated expression "*the rotation* AB" to denote that rotation of the moveable sphere round the axis of the great circle joining AB, which would cause the point A on the moveable sphere to describe the arc AB on the fixed sphere; and we may call the positive or north pole of such a rotation *the positive or north pole of the arc* AB.

It is plain that the rotation AB is the same thing whether the two points A, B be supposed to be on the fixed or moveable sphere. But two successive rotations AB, CD will in general produce a different displacement, according as the four points A, B, C, D are on one or the other sphere.

I now proceed to the theorem in question.

THEOREM. *Let* ABC *be any spherical triangle upon the fixed sphere. Then twice the rotation* AB, *followed by twice the rotation* BC, *produces the same displacement as twice the rotation* AC.

Fig. 1.



Demonstration.—Produce AB, CB (fig. 1) to E and D, making

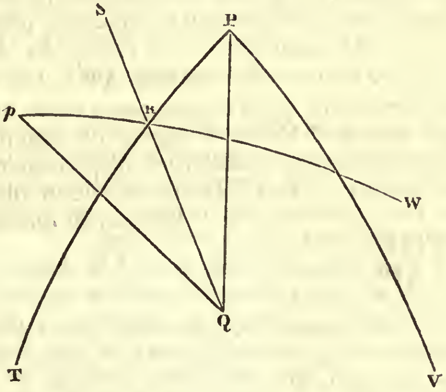
BE = AB and BD = CB. Also produce AC both ways, and make AF = CG = AC. Then because the angles D, E are respectively equal to the angles at C and A, and DE = AC, it is plain that the rotation AE (or 2AB) will carry that arc on the moveable sphere which at first coincides with FA, into the position DE; and then the rotation DC (or 2BC) will carry the same arc from the position DE to the position CG. But this last is the position into which it would have been carried by the single rotation FC (or 2AC); whence the truth of the theorem is manifest.

The following is an equivalent theorem, easily deducible from the preceding. It is worth while, however, to establish it independently.

Let PQR *be any spherical triangle upon the fixed sphere, the letters being so arranged that* Q *is the positive pole of a rotation from* QP *to* QR. *Then, if* P, Q, R *denote the interior angles of the triangle, a positive rotation* 2P *round the pole* P, *followed by a positive rotation* 2Q *round* Q, *produces the same displacement as a positive rotation* $2(\pi - R)$ *round* R.

Demonstration.—Produce PR, and draw PV making an angle VPQ = QPT. Produce QR; draw Qp = QP, making an angle pQR = PQR; join pR and produce it. Then it is plain that the triangles pQR, PQR have their sides and angles respectively equal. Now the first rotation

Fig. 2.



(round P) will carry that arc on the moveable sphere which at first coincides with PT, into the position PV; and the next rotation (round Q) will carry the same arc into the position pW. But this same displacement would evidently have been produced by a single rotation round R through the angle PRp, or 2SRP. Whence the theorem is proved.

It is easy to establish corresponding propositions for the case in which the spherical triangles are drawn upon the *moveable sphere.* In fact, we have only to look upon the displacements supposed in the preceding demonstrations, as *rela-*

tive instead of *absolute*, and consider the sphere there supposed moveable to be fixed, attributing opposite rotations to the sphere there supposed fixed, and we obtain the following theorems:—

Let ABC *be any triangle on the moveable sphere. Then twice the rotation* BA *followed by twice the rotation* CB *is equivalent to twice the rotation* CA.

Referring to figure 1, and considering it as drawn on the *moveable* sphere, we see that this is equivalent to the following proposition, viz. twice the rotation EB followed by twice the rotation BD is equivalent to twice the rotation CA. Whence it is obvious that we may enunciate the theorem as follows:—

Twice the rotation AB *followed by twice the rotation* BC *is equivalent to twice the rotation* DE.

The *second* theorem will take the following form:—

Let PQR *be any triangle on the moveable sphere, the letters being arranged as before. Then a negative rotation* 2P *round* P, *followed by a negative rotation* 2Q *round* Q, *is equivalent to a negative rotation* $2(\pi-R)$ *round* R.

It is also easily seen that in this enunciation we may substitute *positive* for *negative* rotations, provided we adopt the reverse arrangement of the letters P, Q, R, namely, that in which Q shall be the *negative* pole of the rotation from QP to QR.

If in this theorem, whether referring to the fixed or moveable sphere, we suppose the rotations indefinitely small, the *order* of the two component rotations becomes indifferent, and we easily obtain the ordinary propositions concerning the composition of angular velocities.

I shall not, however, enter into details upon this point, as I wish at present to point out the use which may be made of the preceding theory, as affording a direct connecting link between the geometrical laws of the displacements of a rigid system, and the analytical formulæ of Sir W. Hamilton's method of quaternions. In fact, the latter method furnishes an algebraical theory of spherical trigonometry; and we have just seen that the geometrical laws in question depend in a very simple manner on the properties of spherical triangles. I must premise, however, that I have found it convenient to use a system of interpretation of quaternions which differs, to a certain extent, from that employed by Sir W. Hamilton himself. It is possible, therefore, that the two following instances of the application of the method may not be fully intelligible. I give them nevertheless, in the hope that I may be allowed in a future communication to explain the prin-

ciples of interpretation alluded to, which appear to me to possess considerable advantages, at least for some purposes.

Returning to the suppositions made at the beginning of this paper, let us conceive two systems of rectangular axes, having their common origin at the centre; one system being fixed in space, and the other invariably connected with the moveable sphere; the axes being, moreover, so arranged, that the positive axis of Z shall meet the sphere in the *positive* pole of the rotation from X to Y.

Now let ABC be any triangle on the fixed sphere, and let

$$AB=\frac{1}{2}\theta, \quad BC=\frac{1}{2}\theta', \quad AC=\frac{1}{2}\theta''.$$

Also let

$$l, m, n; \quad l', m', n'; \quad l'', m'', n''$$

be respectively the coordinates of the positive poles of the rotations AB, BC, AC. Then if

$$q=\cos\frac{\theta}{2}+\sin\frac{\theta}{2}(il+jm+kn),$$

and q', q'' denote quaternions similarly composed of accented letters, we have, by the principles of the application of quaternions to spherical triangles,

$$q''=q'q. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

On the other hand, we have seen that the rotation $2AC(=\theta'')$ is the effect of the successive rotations 2AB, 2BC; we have therefore the following equations derived from (1.), and defining the rotation which is equivalent to two given successive rotations:—(put, for shortness,

$$k=\cos\frac{\theta}{2}, \quad \lambda=l\tan\frac{\theta}{2}, \quad \mu=m\tan\frac{\theta}{2}, \quad \nu=n\tan\frac{\theta}{2},$$

with similar substitutions for the accented letters; then)

$$\begin{aligned}
k''&=kk'(1-\lambda\lambda'-\mu\mu'-\nu\nu')\\
k''\lambda''&=kk'(\lambda+\lambda'+\mu'\nu-\mu\nu')\\
k''\mu''&=kk'(\mu+\mu'+\nu'\lambda-\nu\lambda')\\
k''\nu''&=kk'(\nu+\nu'+\lambda'\mu-\lambda\mu').
\end{aligned}$$

Formulæ equivalent to these were obtained by Mr. Cayley as analytical results of M. Rodrigues' formulæ for the transformation of coordinates, and published in the Cambridge Mathematical Journal in 1843 (vol. iii. p. 226). Their geometrica significance, however, was not apparent before the d scovery of quaternions. The possibility of representing the effect of successive rotations of a rigid system by a quaternion

product has been since noticed both by Mr. Cayley (Phil. Mag. vol. xxxiii. p. 196) and Mr. Boole (vol. xxxiii. p. 278); I have introduced the above proof of it partly for the sake of future reference, and partly because I am not certain whether Mr. Boole refers to it as a proposition depending upon Mr. Cayley's results, or as the analytical expression of an independent geometrical theorem, under which latter aspect I have here viewed it.

I shall conclude this paper by showing how the formulæ of M. Rodrigues may be established by means of quaternions. The investigation is closely connected with the subject of the preceding observations, and it will be seen to include an explanation of another remarkable analytical result noticed by Mr. Cayley (Phil. Mag. vol. xxvi. p. 142).

Recurring to fig. 1, and recollecting that the radius of the sphere is represented by unity, let l, m, n be the coordinates of the positive pole of AB, and x, y, z the coordinates of the positive pole of AC, both referred to the fixed axes, with which the moveable axes at first coincide. Also let

$$2\mathrm{AB}=\theta, \quad 2\mathrm{AC}=\phi.$$

When the moveable sphere has been displaced by the rotation $\mathrm{AE}=\theta$, let

$$a, b, c; \quad a', b', c'; \quad a'', b'', c''$$

be respectively the direction cosines of the *moved* axes, referred to the fixed axes. The plane of the great circle (on the moveable sphere) which at first coincided with that of AC, now coincides with that of DE, so that x, y, z are also the coordinates of the positive pole of DE referred to the *moved* axes. Hence if ξ, η, ζ be the coordinates of this same pole referred to the *fixed* axes, we have, by the common formulæ,

$$\xi=ax+a'y+a''z$$
$$\eta=bx+b'y+b''z$$
$$\zeta=cx+c'y+c''z.$$

Now let

$$q=\cos\frac{\theta}{2}+\sin\frac{\theta}{2}(il+jm+kn)$$

$$q'=\cos\frac{\phi}{2}+\sin\frac{\phi}{2}(ix+jy+kz)$$

$$q''=\cos\frac{\phi}{2}+\sin\frac{\phi}{2}(i\xi+j\eta+k\zeta).$$

Then, applying the calculus of quaternions to the triangles ABC, BDE, it is easily seen that we have

$$q\,q'\,q^{-1}=q''.$$

If we assume for convenience $\varphi=\pi$, and put

$$l\tan\frac{\theta}{2}=\lambda,\quad m\tan\frac{\theta}{2}=\mu,\quad n\tan\frac{\theta}{2}=\nu,\quad \sec^2\frac{\theta}{2}=\varkappa,$$

and substitute for ξ, η, ζ their values in terms of x, y, z, this equation becomes

$$(1+i\lambda+j\mu+k\nu)(ix+jy+kz)(1-i\lambda-j\mu-k\nu)$$
$$=\varkappa\{i(ax+a'y+a''z)+j(bx+b'y+b''z)+k(cx+c'y+c''z)\}.$$

The first side, developed, is

$$i\{(1+\lambda^2-\mu^2-\nu^2)x+2(\lambda\mu-\nu)y+2(\nu\lambda+\mu)z\}$$
$$+j\{(1+\mu^2-\nu^2-\lambda^2)y+2(\mu\nu-\lambda)z+2(\lambda\mu+\nu)x\}$$
$$+k\{(1+\nu^2-\lambda^2-\mu^2)z+2(\nu\lambda-\mu)x+2(\mu\nu+\lambda)y\}.$$

Observing, then, that the equation must subsist independently of the values of x, y, z, we have

$$\begin{array}{lll}
\varkappa a=1+\lambda^2-\mu^2-\nu^2 & \varkappa a'=2(\lambda\mu-\nu) & \varkappa a''=2(\nu\lambda+\mu)\\
\varkappa b=2(\lambda\mu+\nu) & \varkappa b'=1+\mu^2-\nu^2-\lambda^2 & \varkappa b''=2(\mu\nu-\lambda)\\
\varkappa c=2(\nu\lambda-\mu) & \varkappa c'=2(\mu\nu+\lambda) & \varkappa c''=1+\nu^2-\lambda^2-\mu^2.
\end{array}$$

These are the formulæ in question, as given by Mr. Cayley in the Cambridge Mathematical Journal*.

The preceding process affords a striking example of the power of the method of quaternions in reducing complex geometrical questions of a particular class to algebraical calculation. The reader will probably have perceived that I consider the quaternion

$$\{\cos\vartheta+\sin\vartheta(il+jm+kn)\}\rho$$

(where $l^2+m^2+n^2=1$) as representing the rotation of a *line* or *radius vector* ρ through an angle ϑ in a plane perpendicular to the axis whose direction cosines are l, m, n. In a future paper I propose, with the editors' permission, to explain the system of interpretation which leads to this result, and to show that it represents the theory of quaternions as a natural and, indeed, necessary extension of the common geometrical algebra of two dimensions.

Oxford, April 30, 1850.

* It is right to state that my knowledge of the contents of M. Rodrigues' memoir is derived solely from the paper just mentioned, as I have not at present an opportunity of referring to the fifth volume of Liouville's journal.

LVI. *On a new and curious Application of the Permanence of Impressions on the Retina.* *By* M. J. Plateau, *Member of the Royal Academy of Belgium**.

[With a Plate.]

TWO discs of white paper, of sufficient strength, are to be cut out, one 30 and the other 35 centimetres in diameter. The first is divided into eight equal sectors; two opposite sectors are then painted red, and two others blue, employing for this purpose fine gum colours; two other opposed sectors are afterwards covered with a very opake black, and a circular space of 4 centimetres in diameter, in the centre of the disc, is covered with the same black; finally, the two last sectors are left white (see Plate II. fig. 1, in which the colours red and blue are indicated by the shading and the letters *r* and *b*). When this is done, a colourless varnish is laid upon the disc, which penetrates into the pores of the paper, and gives more transparency to the white and coloured parts†. In the other disc are made two opposite openings, likewise in the form of sectors, but which extend to only 3 centimetres of the circumference of the disc, and whose angular width is only three-fourths of that of the sectors of the other disc; their extremities adjoining the centre also leave a distance between them of 4 centimetres; this disc is blackened all over (see fig. 2).

These discs are fixed respectively at their centres, by means of nuts, upon two small brass pulleys 3 centimetres in diameter. These last are placed upon a support so that the discs are vertical, parallel to one another, and distant 3 centimetres from each other, and so that the two axes are in the same straight line. These pulleys are furnished with cords which pass over two other larger pulleys; these are of wood; they are both 15 centimetres in diameter, and are fixed upon a common axis, which is furnished with a small handle. Lastly, the cords are arranged so that the two brass pulleys, and consequently the two discs, turn in the same direction. This system is similar to that of the *anorthoscope*, as it has been presented to the public, except the equality in diameter of the brass pulleys, the community of direction of their motions, the less number of the openings of the black disc, and the greater width of these openings. Like the anorthoscope also, the present instrument must be employed in the evening, and strongly illumined by a good lamp

* From the *Bulletin de l'Acad. de Bruxelles*, vol. xvi. p. 424.

† In order that, in the disc thus varnished, the tints of the coloured parts may remain very bright when observed by transmission, it is well for the colours to be applied on both sides of the paper.

conveniently placed behind the coloured disc; lastly, the observer must also place himself on the other side of the apparatus, that is to say, opposite to the black disc, keeping his eyes on a level with the centres of the discs, whilst another person turns the handle with sufficient rapidity. The distance of the observer from the apparatus must be a metre at least.

With this arrangement, let us imagine, for an instant, that the diameters of the two brass pulleys are mathematically equal, that the same is the case with the two wooden pulleys, and that the thicknesses of the two cords are also exactly alike. Then the velocities of the two discs will be perfectly equal; and as they are in the same direction, it is clear that if, at the commencement of the movement, the two openings correspond, for example, to the red sectors, they will continue to correspond with them indefinitely; so that, if the velocity is sufficient, the observer will simply see the whole circular space traversed by the openings coloured a uniform red, and the effect of the apparatus will be limited to this. But this perfect equality of the velocities will not be realized; for supposing the pulleys to have been made with such care that their diameters might be regarded as perfectly equal, and admitting moreover that the two cords, taken from the same piece, have exactly the same thickness, special precautions would still be requisite for them to have exactly the same tension; and a very small difference in this respect suffices to alter in some degree the equality of their thicknesses, and consequently that of the velocities. There will therefore be in general a small inequality between the velocities of the two discs, and upon this inequality is founded the illusion here in question.

In fact, let us suppose, in order to fix our ideas, that the colours are ranged in the order of figure 1, that the discs turn in the direction indicated by the arrows, and that the black disc has a small excess of velocity. In this case, let us arrange matters so that before putting the apparatus in motion, the middle of the width of the openings may correspond with the middle of that of the black sectors of the other disc. It is then clear that at the commencement of the movement the observer will only see a completely black surface. But the relative position of the openings and of the black sectors changing by degrees, by reason of the small excess of velocity of the first, after a certain time the openings will begin encroaching a little upon the red sectors, and consequently the observer will see the black surface become uniformly coloured with a slight tinge of red; then the encroachment of the

openings upon the sectors of this colour continually increasing, the tint of the apparent surface will become more and more vivid, and will pass at last into a brilliant red when the openings project entire upon the red sectors. This tint will continue without alteration until the openings begin to encroach upon the white sectors; the red will then begin to grow pale, will pass slowly into a rose colour, becoming gradually of a lighter shade, and finally to white. This last will then gradually change into a blue, more and more vivid, which after some time will begin to darken, giving place insensibly to black. Lastly, if the movement be continued, the phænomena will be reproduced in the same order.

The tints thus produced are very beautiful, without having too much brilliancy; their uniformity is perfect, and the passage from one to the other is effected with extreme delicacy. This illusion then presents, within certain limits, a sort of realization of the ocular harpsichord of Father Castel, and the persons to whom I have shown it have appeared to derive from it great pleasure.

In conclusion I would urge this point, that the success of the experiment entirely depends on the establishment of a suitable inequality between the velocities of the two discs. To attain this, it is necessary that, in the construction of the instrument, the diameters of the two brass pulleys should be made as perfectly equal as possible, as well as those of the two wooden pulleys, and that great care should be taken to give the same tension to the two cords, which should be taken from the same piece; the small differences which will always exist, in spite of these precautions, will produce, unless by a very peculiar chance, a sufficient inequality between the two velocities; for, it will be understood, it is necessary that this inequality should be the least possible.

LVII. *Second Note upon some new and curious applications of the Permanence of Impressions on the Retina. By* J. Plateau, *Member of the Royal Academy of Belgium**.

[With a Plate.]

When, under the name of *Anorthoscope*†, I described the instrument intended to produce a peculiar kind of anamorphoses by means of two discs rotating rapidly one before the other, the hind one of which is transparent and bears distorted figures, whilst the front one is opake and pierced with a small number of narrow slits, I made no allu-

* From the *Bulletin de l'Acad. de Bruxelles,* vol. xvi. p. 588.

† *Bulletin de l'Académie,* vol. iii. p. 7, year 1836.

sion to the two velocities, their relative direction, and the form of the slits. In the instrument as constructed according to my directions for the public, the velocity of the transparent disc is to that of the opake disc as 4 to 1, and these velocities are in opposite directions; lastly, the slits are straight, and directed from the centre of the disc to the circumference.

With these elements, as shown by the instrument in question, the distorted figure is single, it is angularly dilated, and it gives rise to five regular figures symmetrically arranged around the centre of the disc; but it is clear that other arrangements would lead to other results. Now as no one, that I am aware, has hitherto varied these arrangements, and as, on changing them in part, we arrive in certain cases at results which appear to me curious, I proceed to examine the subject in some detail.

To simplify the considerations, I will suppose the slits excessively narrow, so that they may be regarded as simple lines. This being laid down, I shall here call to mind that the regular figure obtained by the simultaneous movement of the two discs is composed of the aggregate of the impressions left on the eye by the points of the distorted figure seen through the same slit in the successive positions of the latter. Now, if the slit is considered in one of these positions in particular, all the points of the distorted figure which correspond to it at that instant presented to the eye, are perceived simultaneously, and consequently belong, with their relative positions, to the regular figure; that is to say, they are in the latter ranged identically in the same manner upon a line of the same form as the slit. Reciprocally, then, if, after having drawn upon a sheet of paper the regular figure which it is desired to reproduce by means of the apparatus, a line be traced upon the latter which represents the slit in one of its positions, all the points in which this line will cut the figure should be found placed identically in the same manner upon a similar line in the drawing of the distorted figure; and the same will take place with any number of similar lines, traced upon the drawing of the regular figure, and representing so many successive positions of the slit. The series of points which are found respectively upon these lines, will then have all their identical corresponding points upon the drawing of the distorted figure; but these last will not occupy among them the same relative positions as upon the first drawing, and in this will consist the deformation.

To fix our ideas and avoid complication, let us suppose, in what follows, that the slits are rectilinear, and directed according to the radii of the disc. This being the case, we

proceed to study successively the systems in which the two discs turn in contrary directions, and those in which they turn in the same direction.

I. *Velocities in contrary directions.*

Let us imagine a slit in one of its positions, which for greater simplicity we will suppose vertical, and the radius of the transparent disc, which is seen at this moment through this slit, let us call r; the impression left on the retina by the series of the points of the distorted figure ranged on this radius r, will belong to the regular image. Let us then imagine the slit in a subsequent position, making with the first any angle which we will designate by α. In this new position there will be another radius of the transparent disc, which we will call r', behind the slit, and the impression produced in the eye by the succession of the points of the distorted figure ranged on this second radius, will belong also to the regular image. In this image the two series of points will comprise therefore between them the angle α. But whilst the slit, quitting the vertical, has advanced up to its meeting the radius r' whilst describing this angle, the radius r, also quitting the vertical, has described, in a contrary direction, another angle which we shall designate by β; whence it follows that at the instant when the slit comes before the radius r', at an angular distance α from the vertical, the radius r is, on the opposite side, at an angular distance β from this same vertical, and that, consequently, upon the transparent disc, the radii r and r' comprise between them an angle equal to $\beta+\alpha$.

Thus, two series of points respectively ranged upon two radii traced upon the drawing of the regular figure, and comprising between them the angle α, will have for correspondents upon the drawing of the distorted figure, two series of points ranged in the same manner upon two radii comprising between them the greater angle $\beta+\alpha$; and, consequently, the deformation will consist simply of an angular dilatation of the regular figure.

It is easy to find the general expression of the relation $\frac{\beta+\alpha}{\alpha}$. In fact, this relation may be stated under the form $\frac{\beta}{\alpha}+1$; now the angle β is evidently to the angle α as the velocity of the disc which bears the deformed figure is to that of the black disc; if, then, we designate the first by V_d and the second by V_n, and if we represent by M the relation $\frac{\beta+\alpha}{\alpha}$,

we shall have

$$M = \frac{V_d}{V_n} + 1, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

a quantity which depends only on the relation between the velocities of the two discs.

From the above it is seen, that, for a given relation between the two velocities, the construction of the distorted figure will be effected by the following process:—1. Tracing upon the drawing of the regular figure which it is desired to reproduce, a series of radii sufficiently near together. 2. Tracing the same number of radii upon the disc which is to bear the distorted figure, but so that the angle comprised between each of these latter radii and that which follows it be to the angle comprised between the two corresponding radii of the regular drawing in the relation M. 3. Transferring successively upon each of the radii of the disc the series of points ranged upon the corresponding radii of the regular drawing, placing these points at the same respective distances from the centre. 4. Lastly, connecting with each other, by suitable lines, the points thus distributed. For example, in the published anorthoscope, the velocity of the transparent disc is fourfold that of the black disc, which gives $M=5$; and, in fact, the construction of the drawings of that instrument consisted simply in rendering the angular distances five times greater than in the regular figures which were to be reproduced. It is seen, by the above general expression of the relation M, that the deformation will be greater in proportion as the velocity of the transparent disc is more considerable with relation to that of the black disc.

We may substitute, in the formula (1.), for the quantity M its equal $\frac{\beta+\alpha}{\alpha}$; hence we shall consequently have

$$\beta+\alpha = \alpha\left(\frac{V_d}{V_n}+1\right),$$

an expression in which $\beta+\alpha$ is evidently the angular distance which separates the radius r of the slit, when the latter has described the angle α. Now, if this latter angle is such that the angular distance $\beta+\alpha$ has for its measure the entire circumference, the slit will occur again before the radius r. If, then, we designate by α' the value of α which fulfills this condition, and if, in order to simplify, we take the circumference as unity, the condition in question will give

$$\alpha'\left(\frac{V_d}{V_n}+1\right)=1,$$

whence

$$\alpha' = \frac{1}{\frac{V_d}{V_n}+1}, \quad . \; . \; . \; . \; . \; . \; . \quad (2.)$$

a value necessarily less than unity. Consequently, if we suppose that the radius r is that which contains the first point of the distorted figure, that figure will pass entire behind the slit, whilst the latter describes the angle measured by the fraction

$$\frac{1}{\frac{V_d}{V_n}+1},$$

and a regular, perfect figure will thus be produced in this same angle. But as the slit then coincides again with the first point of the distorted figure, a new regular figure will also be produced in a following angle equal to the first, and so on. The figure is multiplied, then, whilst becoming regular; and if the angle of which we have just spoken is measured by an aliquot part of the circumference, which evidently requires that the relation $\frac{V_d}{V_n}$ be an entire number, the regular figures will be found ranged symmetrically round the centre, and will be reproduced in the same positions at each revolution of the slit. It is clear that the number of these figures will then be equal to

$$\frac{V_d}{V_n}+1.$$

In the published anorthoscope we have

$$\frac{V_d}{V_n}+1=5;$$

and there are produced, in fact, five identical regular figures, symmetrically arranged around the centre.

The velocities must always be taken such, that the relation $\frac{V_d}{V_n}$ be an entire number; for otherwise, when the slit shall have made one revolution, it is clear, from what precedes, that it will not be found again in coincidence with the first point of the distorted figure, and that, consequently, the regular figures afterwards produced will not be superposed upon the first, so that there will be confusion.

If it is desired that each regular figure be seen completely separated from the neighbouring ones, it must evidently be necessary, when the drawing of it is made, that it be comprised

in an angle less than that which has for measure the fraction

$$\frac{1}{\frac{V_d}{V_n}+1}.$$

This is what has been done, for example, in the published anorthoscope, with regard to the figure which represents a lady holding a parasol. If the drawing exceeds the angle in question, the distorted figure will be found folded upon itself in such a manner that one of its extremities partly conceals the other, and each of the regular figures produced will have one of its extremities partly concealed by the other extremity of its adjoining one. This, for example, may be seen in the published anorthoscope, with respect to the figure representing a galloping horse. It is clear that it will be usually advantageous to arrange matters in this manner.

Lastly, we have hitherto reasoned on the hypothesis in which the black disc should present only a single slit; but it is easy to show that a number of equidistant slits may be pierced in this disc equal to the relation $\frac{V_d}{V_n}$. In fact, if we take again as initial positions of the two discs those in which one slit corresponds to the first point of the distorted figure, it is evident that, after one revolution from this point, the slit will have made a portion of a revolution measured by the fraction $\frac{V_n}{V_d}$; now, if at this moment a second slit comes before the point in question, the regular figures produced by the latter will necessarily be superposed upon the first. As many slits, then, may be pierced in the black disc as the times that the above fraction is comprised in unity, that is to say $\frac{V_d}{V_n}$ slits, it always being understood that this latter quantity is a whole number. It is thus that, in the published anorthoscope, for which we have $\frac{V_d}{V_n}=4$, the black disc presents four slits.

II. *Velocities in the same direction.*

Let α and β, as before, be the angles described at the same time, starting from the vertical, by a slit and by a radius r of the transparent disc, and let also r' be the radius before which the slit comes in its second position. The angle α will still be that comprehended, in the regular image, by the two series of points respectively ranged upon the radii r and r'; but here the angles α and β being on the same side of the vertical, the

angle comprised upon the transparent disc between the radii r and r', will evidently be equal to the difference of these angles. Thus, two radii traced upon the drawing of the regular figure, and comprising between them the angle α, will have for correspondents, upon the drawing of the distorted figure, two radii comprising the angle $\alpha-\beta$, an angle the value of which may be positive or negative; the relation of these two angles will then be

$$\frac{\alpha-\beta}{\alpha}=1-\frac{\beta}{\alpha}=1-\frac{V_d}{V_n}.$$

If we designate it again by M, the construction of the distorted figure will be given by the expression

$$M=1-\frac{V_d}{V_n}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3.)$$

But the results of this construction will be of a different nature, according as we have $V_d < V_n$ or $V_d > V_n$; that is to say, according as the velocity of the transparent disc is greater or less than that of the black disc. We shall now proceed successively to a consideration of these two cases.

First case: $V_d < V_n$.

In this case the relation M will necessarily be less than unity; whence it follows that the distortion will always consist of an angular contraction, a contraction which will be stronger as the velocities approach nearer to one another.

If we designate by γ the angle comprised between the two radii which pass respectively by the first and by the last point of the regular figure, and by γ' the angle comprised between the two radii which pass in the same manner by the extreme points of the distorted figure, we shall evidently have $\frac{\gamma'}{\gamma}=M$, whence

$$\gamma'=M\gamma. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4.)$$

Now it is evident, that here, in one revolution of the slit, only a single regular figure will be manifested. In fact, for this figure to be multiple, it would be necessary for the slit to recur several times in its revolution, in coincidence with the first point of the distorted figure; now, according to a similar coincidence, the point in question, in virtue of the less velocity of the transparent disc, will remain behind the slit; so that this last will have made its revolution before it, and consequently no other coincidence can be produced. Nothing, then, limits the angular extent which the regular figure may have; and consequently, when this figure is constructed to de-

form it afterwards, it may be taken quite arbitrarily as to position and size, provided, be it well understood, that it is comprised in a circle equal in diameter to the transparent disc. This regular figure may, for example, be a head equal in height to the diameter of the disc. Only, the pulleys to which the discs are fixed, and the rod which bears these pulleys, will conceal a small portion of the image. The greatest angle which the regular figure can occupy will then be measured by an entire circumference. Consequently, if we again take the circumference for unity in the measure of the angles, and designate by A the greatest angle which the distorted figure can occupy, the equation (4.) will give, making $\gamma=1$ and $\gamma'=A$,

$$A=M; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5.)$$

thus the extent of the distorted figure cannot exceed an angle having for measure the fraction M.

Although one entire revolution of the slit can, as we have seen, produce only a single regular figure, yet the figures which are produced in the subsequent revolutions must be all superposed on the first, without which there would be confusion. But this requires that the successive coincidences of the slit and the first point of the distorted figure be effected at the same spot; or, in other words, that in the interval of one coincidence to the following, the slit make either one complete revolution, or an entire number of revolutions. Let us then seek the condition which, for this to happen, the relation $\frac{V_d}{V_n}$ of the two velocities must satisfy.

Let us suppose this relation reduced to its simplest expression, or, in other words, let us assume to represent the two velocities, two numbers which have no common factor. As in the case of the velocities of contrary directions, let α' be the angle described by the slit from one coincidence with the first point of the distorted figure up to the ensuing coincidence. The mode of reasoning which we have employed to arrive at the expression (2.), will lead us, in the present case, to the relation

$$\alpha'\left(1-\frac{V_d}{V_n}\right)=1,$$

which will give

$$\alpha'=\frac{1}{1-\frac{V_d}{V_n}}=\frac{V_n}{V_n-V_d}.$$

But as, from one coincidence to the other, the slit must ac-

complish one revolution or an entire number of revolutions, it will be necessary that the angle α' be equal to unity or to any entire number. Now we cannot suppose $\alpha'=1$; for, according to the above expression, there would result from it $V_d=0$; thus the second coincidence can only be produced after several revolutions of the slit. Now, as the numbers V_n and V_d have no common factor, it is clear that the quantity $\frac{V_n}{V_n-V_d}$, or α', can only be equal to an entire number, if we have $V_n-V_d=1$. We arrive, then, lastly, at this conclusion, that the velocities should be taken such, that the numbers which represent them differ among themselves only by a unit. We shall then have simply $\alpha'=V_n$; that is to say, from one coincidence to another, the slit will accomplish a number of revolutions equal to V_n.

We remark, moreover, that in this case the value of M given by the expression (3.) is simplified, and becomes

$$M=\frac{1}{V_n}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6.)$$

The expression (5.) will also consequently become

$$A=\frac{1}{V_n}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (7.)$$

This being settled, let us examine more closely what passes in the successive revolutions of the slit. If we always start from a coincidence, it is at once evident that, in the first of these revolutions, a complete regular figure will have been produced; or, in other words, that the slit will have passed before the whole of the distorted figure. In fact, according to the manner in which we have arrived at the formula (3.), if we suppose that after a coincidence with the first point of the distorted figure the slit has described an angle α, the quantity M will represent also the relation between the angular distance which then separates the slit from the point in question, and that angle α; if, then, α constitutes an entire revolution, and is thus measured by unity, the above angular distance, or, in other words, the portion of the transparent disc before which the slit will have passed from the coincidence, will then be represented by M, and consequently, in the present case, by $\frac{1}{V_n}$. But, according to the expression (7.), the fraction $\frac{1}{V_n}$ is the measure of the largest angle which the dis-

torted figure can occupy; then, as I have said, this figure will have been crossed entirely by the slit.

According to this, since the slit will only be found in coincidence with the commencement of that angle after $V_n - 1$ new revolutions, it follows that, during these, it will pass before the free part of the disc, and that nothing longer will be seen; then, that a second regular figure will be produced in a position identical with the first, to be followed by a new interval in which nothing will be seen, and so on. This would constitute a serious inconvenience, did not a very simple means present itself to obviate it. This means consists in dividing the transparent disc into V_n equal angles, and repeating, in each of them, the drawing of the distorted figure. Then, in fact, it is clear that after each of its revolutions, the slit will coincide with the origin of one of these angles which it will sweep entire in the following revolution, so that the regular images will be produced without interruption, and will all be mutually superposed. Another advantage will hence result; that in general the anamorphosis will be much more difficult to decipher. Thus, whilst for velocities in opposite directions the distorted figure is single and the regular image multiple, it is the contrary in the case which we are examining; that is to say, the distorted figure is multiple and the regular image single.

Lastly, let us seek what number of slits may be pierced in the black disc. For each of these slits to produce identically the same effect, it will suffice that when, after a coincidence between one slit and the first point of one of the distorted figures, the first point of the following distorted figure shall reach the same spot, another slit is come before it. Now, from one of these two positions to the other, the transparent disc has moved over an angle measured by the fraction $\frac{1}{V_n}$; and as the angle, the slit of which has turned at the same time, is to the preceding one in the relation of the velocity of the black disc to that of the transparent disc, this angle will evidently have for measure the fraction $\frac{1}{V_d}$. Such, then, is the value of the angle by which the second slit should be removed from the first; and as the same thing will take place with regard to the third slit in relation to the second, and so on, it is clear that the total number of the slits will be equal to V_d.

We will illustrate all this by an example. Let us suppose that the velocity of the transparent disc is to that of the black disc as 3 to 4, numbers which satisfy the condition that we have established, namely, only differing from each other by

one unit. We shall then have $V_d=3$, and $V_n=4$, which will give $M=\frac{1}{4}$; whence it follows that the distorted figure will be constructed by reducing the angular distances between the different points of the regular figure to a fourth of their respective values. We shall have, moreover, $A=\frac{1}{4}$; that is to say, the distorted figure will be found comprised in a right angle, and must be repeated four times. In fine, three slits may be pierced in the black disc. With these elements, we start from the coincidence between one slit and the first point of one of the partial distorted figures. When this slit shall have completed one revolution, the point in question will only have made three-fourths of its revolution, so that it will be removed from the slit an angular distance equal to a right angle. The slit will then have passed before the whole of one of the distorted figures, and consequently a complete regular figure will have been produced. But then the slit will be found before the first point of the following distorted figure, so that its second revolution will cause a second regular figure superposed upon the first, and so on. Moreover, when, after one coincidence, a slit shall have effected one-third of a revolution, another slit will be found in the position which the first occupied at the instant of that coincidence; but also, during this third of a revolution, the transparent disc will have made a fourth of its revolution: the second slit will then correspond to the first point of the succeeding distorted figure, and consequently these two slits will produce identical effects; in short, the same reasoning applies to the third slit with relation to the second, and it is seen that an uninterrupted succession of regular images will result from this system, which will be completely superposed upon one another.

In order to form an idea of the kind of deformed drawings which the case under our consideration gives, I suppose that with the above elements it is wished to obtain, for a regular figure, the word LOI written in white letters upon a black ground (Plate III. fig. 1); the construction of the distorted figures will then give the fanciful drawing represented by fig. 2; and we see that unless accustomed to this kind of anamorphosis, it would be very difficult to divine, from the inspection of this drawing, the regular figure which it will produce.

It remains for us to offer two remarks with respect to these figures. In the first place, let us consider a radius traced upon the drawing of the regular figure, and let us take, for example, that which, in the straight position of this figure, is

directed vertically from below upwards, starting from the centre. Let us also consider a slit at the moment when it is equally directed from below upwards. If at this instant the slit is situated before the radius of one of the distorted figures corresponding to the above radius of the regular figure, it is evident that the regular image produced will be seen in the straight position; but if the slit coincides with another radius of the distorted figure, this last will then occupy, in the regular image, the vertical position, and consequently this image will be seen inclined in one direction or the other, or even reversed. Now we shall recall here*, that, unless by a quite peculiar chance, the system of pulleys, however carefully constructed, will never realize in an exact manner the relation which has been assigned to the velocities of the two discs; whence it follows, that, when the slits shall one after the other attain the vertical position from below upwards, they will coincide with radii which occupy, in the partial distorted figures, somewhat different positions, so that the regular images successively produced will not be exactly superposed; but if the apparatus is well made, this displacement of the images will be excessively small, and there will result from it only the continued sensation of a single regular image turning very slowly around the centre, an image which, consequently, will necessarily pass by the straight position. For the rest, this movement of the regular image may be avoided, by substituting for the system of pulleys a system of cog-wheels; and then if, by attaching the transparent disc upon its axis, it is placed so as to fulfill the condition indicated above, the image will occupy the straight position, and will maintain it invariably.

In the second place, to observe the image, the spectator must stand at a certain distance from the apparatus, so that the prolongation of the axes of the discs shall pass in the middle of the interval between the two eyes.

These remarks apply equally to the following case:—

2nd Case: $V_d > V_n$.

Here the formula (3.) gives for M a negative value. In order to interpret this change of sign, we resume the reasoning which led us to the formula in question. We start always from a vertical position of the slit and of the radius r, and suppose that this slit and this radius proceed afterwards toward the right. Then, when the slit, after describing the angle α, has come before the radius r', the image of the latter will be seen on the right of that of the radius r. But since the transparent disc turns more quickly than the black disc, the radius

* See the previous note at p. 434 of this number.

r' must have advanced towards the slit; whence it follows that, upon the transparent disc, this radius r' is to the left of the radius r. Hence it results, that all the points of the distorted figure situated on the same side of the vertical will have their correspondents, in the regular image, situated on the other side of this same vertical, and that consequently the angular distances will be measured in opposite directions in the distorted figure and in the regular figure. It is clear, then, that one of these figures will be inverted with reference to the other; in other words, all that in the one is found to the right of the vertical, is in the other to the left of this same vertical, and *vice versâ*. It is clear now on what depends the negative sign of M; for this quantity designating the relation between the corresponding angular distances in the two figures, if we take as positive those which belong to one of these figures, we must, on account of the opposition of direction, consider as negative those which belong to the other, and consequently the relation will take the sign minus.

The absolute value of the relation in question is

$$\frac{V_d}{V_n} - 1.$$

Now, as the quantity $\frac{V_d}{V_n}$ is only limited to the condition of being superior to unity, it is clear that three circumstances may present themselves; namely,

$$\frac{V_d}{V_n} - 1 > 1, \quad \frac{V_d}{V_n} - 1 < 1,$$

or

$$\frac{V_d}{V_n} - 1 = 1;$$

which come back to those:

$$V_d > 2V_n, \quad V_d > V_n \text{ and } < 2V_n,$$

or lastly,

$$V_d = 2V_n.$$

Let us begin by examining the first of these partial cases; in other words, let us suppose that the velocity of the transparent disc exceeds the double of that of the black disc. Then the absolute value of M exceeding unity, it follows that the distorted figure will be, as for the systems of opposed velocities, angularly dilated with relation to the regular figure; and by reasonings analogous to those we have employed with regard to these systems, we shall recognize without difficulty that the regular image will also be multiple; lastly, we shall ascertain

that, for the result produced in a revolution of one of the slits to be superposed on those of the preceding revolutions, the relation $\frac{V_d}{V_n}$ must also be an entire number. Let us take, for example, the velocity of the transparent disc equal to six times that of the black disc. The absolute value of M will then be 5, and consequently the angular dimensions of the distorted figure will be, as in the published anorthoscope, five times those of the regular figure. This distorted figure then will be constructed in the same manner, the inversion excepted, as in the system of the published anorthoscope; whence it follows, that if, for example, we by transmission look at one of the distorted figures which belong to this system, we shall have that which will produce the same regular figure in the new system, on turning the disc, that is to say, by placing opposite the eye the face of this disc which was first turned toward the light. Moreover, in this new system, the regular figure will be likewise repeated five times around the centre; for when, after a coincidence between one slit and the first point of the distorted figure, this slit shall have performed one-fifth of a revolution, the point in question will have performed six-fifths of a revolution, that is to say, one revolution plus one-fifth, so that it will be found behind the slit, and consequently a regular figure will have been produced. As to the number of the slits to pierce in the black disc, they must be six instead of four; for when, after a coincidence between one slit and the above point, this point shall have accomplished one revolution, the slit will only have reached a sixth of its revolution.

According to what we have stated above, it is seen that if we desire, with the same distorted figures, to obtain absolutely identical results in the two systems, it will suffice, in order to employ them in the second, to invert the discs which had been constructed for the first; that is to say, that each of these discs shall be attached to the pulley, so that the face which, in the first system, was turned towards the black disc, should on the contrary be turned towards the lamp; moreover, it will be necessary in one of the systems to employ a black disc pierced with four slits, and in the other a black disc pierced with six slits. If, in passing from one of these systems to the other, the inversion of the transparent discs is not effected, it is clear that it is then the regular figures which will return; if, for example, we employ successively in the two systems, and without inverting it, the distorted figure intended to give, as a regular result, horses at a gallop, the horse which shows itself on the upper part of the image will have, in one of the systems,

the head to the right and the tail to the left, and in the other, the head to the left and the tail to the right.

We arrive, then, at a curious conclusion, namely, that the same transparent discs may be employed with two systems of entirely different velocities. Now it is clear that this conclusion is not limited to the above example, and that for every system of velocities in contrary directions, there may likewise be substituted a system belonging to the partial case under our consideration, and *vice versâ*. We shall evidently obtain the general relation between the proportions of the velocities in these two equivalent systems, by equalling to the value of M given by the formula (1.) the value of this same quantity given by the formula (3.), and taken with contrary signs. If, then, in this last we designate the velocities by V'_d and V'_n, we shall have

$$\frac{V'_d}{V'_n} - 1 = \frac{V_d}{V_n} + 1,$$

whence we shall deduce

$$\frac{V'_d}{V'_n} - \frac{V_d}{V_n} = 2; \quad . \quad . \quad . \quad . \quad . \quad . \quad (8.)$$

and it is seen that, as we have said, if the proportion of the velocities of one of the two systems is a whole number, that of the velocities of the other system will be so likewise.

Let us now pass to the second partial case, that is to say, let us suppose V_d to be comprised between V_n and $2V_n$. Then the absolute value of M will be less than unity, and consequently the distorted figure will be angularly contracted with relation to the regular figure. Following always the same mode of reasoning, we shall see that if V_d only exceeds V_n by one unit, we shall be able to repeat the distorted figure, as in the case of the systems in which, on the contrary, V_n exceeds V_d by one unit; so that there will be again two different systems which may be substituted one for the other. For example, if we take $V_d = 5$ and $V_n = 4$, the absolute value of M will be $\frac{1}{4}$; so that, as in the drawing of the figure 2, the angular dimensions of the distorted figure will be four times less than those of the regular figure, and we shall ascertain without difficulty that the distorted figure may likewise be repeated four times; also the drawing of the figure 2 will produce identically the same regular figure, whether it be employed with the system $V_d = 3$ and $V_n = 4$, or with the system $V_d = 5$ and $V_n = 4$, provided that, in the second, the disc which bears it be reversed. For the second system, however, it will

be necessary, as it is easy to convince oneself, to substitute for the black disc pierced with three slits another black disc pierced with five slits.

Designating again by V'_d and V'_n the velocities of the two discs relative to the partial case which we have just examined, we shall have evidently, for the general relation between two equivalent systems, one of which would belong to this same case, and the other to the case in which, the velocities being always in the same direction, that of the transparent disc would be less than that of the black disc,

$$\frac{V'_d}{V'_n} - 1 = 1 - \frac{V_d}{V_n},$$

which gives

$$\frac{V'_d}{V'_n} + \frac{V_d}{V_n} = 2. \quad . \quad . \quad . \quad . \quad . \quad . \quad (9.)$$

In the system to which the term $\frac{V_d}{V_n}$ of this expression is referred, V_n must, we know, exceed V_d only by one unit; we may then in the place of V_d substitute $V_n - 1$, and the above expression may then be stated under the form

$$\frac{V'_d}{V'_n} = \frac{V_n + 1}{V_n},$$

whence it is seen that, in the supposed relation $\frac{V'_d}{V'_n}$ reduced to its simplest expression, V'_n will be equal to V_n, and V'_d will be a unit greater.

There now remains the third partial case, namely $V_d = 2V_n$. This is of an entirely peculiar nature, and seems to me very remarkable. The absolute value of M is then, in fact, equal to unity; so that the angular dimensions of the distorted figure are equal to those of the regular figure, or, in other words, the deformation consists only in a reversing of this last figure. Reciprocally, then, if upon the transparent disc is drawn any regular figure, the instrument will reproduce this same figure, but reversed, that is to say, having on the left what on the disc is to the right, and *vice versâ*. If, for example, upon the transparent disc there be drawn a head viewed in profile and looking to the right, the instrument will reproduce identically this same head, but turned so as to look to the left. In short, if the drawing traced upon the transparent disc be such that its right half is symmetrical to its left half, if, for example, this drawing represents a head seen in front and illumined in front, or a word formed of letters placed symmetrically, and which do not change by reversing, such as the

Latin word TOT, or again a number composed of figures under the same conditions, such as the number 808, the instrument will reproduce each of these figures with a complete identity, and we shall thus have a new and very curious means of making an object in rapid motion appear to be unmoved.

It is clear that with this system the number of the slits can only be two; for when the first point of the figure, starting from its coincidence with a slit, shall have accomplished a revolution, the slit will have performed a half-revolution, so that, for a second coincidence then to take place, the succeeding slit must be situated opposite to the first.

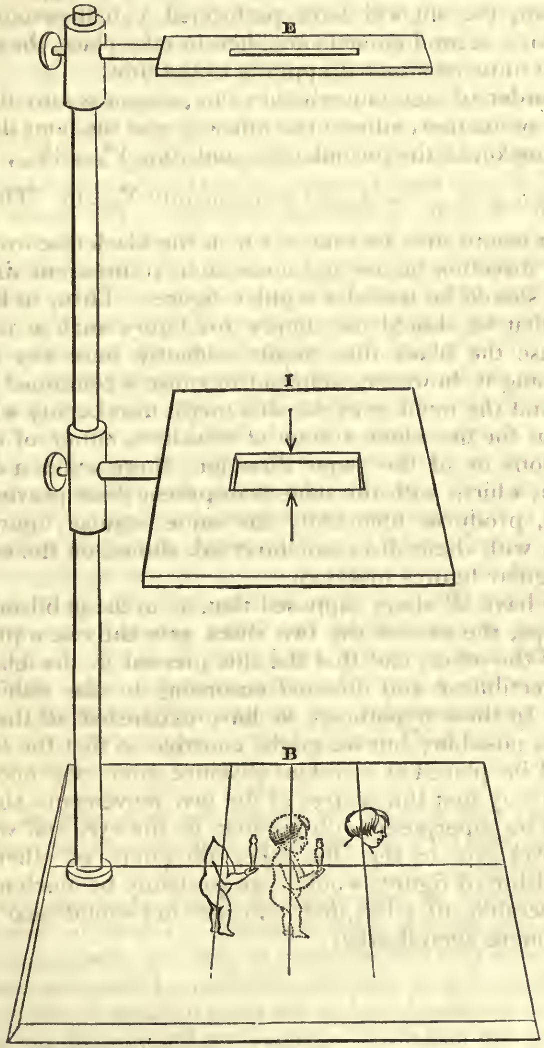
In order to ascertain whether this system is also the equivalent of another, either of a contrary or of the same direction, let us make, in the formulæ (8.) and (9.), $V'_d = 2V'_n$; the two will then give $\frac{V_d}{V_n} = 0$, and consequently $V_d = 0$. This other system would then be that in which the black disc would turn in any direction before an immoveable transparent disc upon which should be traced a regular figure. Then, in fact, it is clear that we should see simply this figure such as it is. In this case the black disc might evidently have any velocity, supposing it, however, sufficient to cause a continued impression, and the number of the slits might also be any whatever.

Thus for the whole system of velocities, either of opposite directions or of the same direction, there exists a different system, which, with the same transparent discs previously inverted, produces identically the same regular figures, and which, with these discs not inverted, shows, on the contrary, the regular figures inverted.

We have all along supposed that, as in the published anorthoscope, the axes of the two discs were the one a prolongation of the other, and that the slits pierced in the black disc were rectilinear and directed according to the radii of this disc. In these hypotheses we have exhausted all the combinations possible; but we might contrive so that the two axes should be placed at a certain distance from one another, in such a way that the centres of the two movements should no longer be superposed with relation to the eye, and we might moreover give to the slits other directions or other forms. The distorted figures would then probably be much more undecipherable still; but their construction would also become much more complicated.

LVIII. *On the Phantascope.* *By* Prof. J. Locke*.

As many persons find it difficult, if not impossible, to converge the optical axes (or in common phrase to look "cross-eyed") for the purpose of making the experiments lately described by me† under the head of "binocular vision," I have invented an instrument which enables all persons to succeed in obtaining the chief results. It is very simple,



E. Eye-screen. I. Index-screen. B. Base-board.

* From Silliman's Journal for March 1850.

† Phil. Mag. vol. xxxiv. p. 195.

having neither lenses, prisms nor reflectors, the object being in general the same as holding the finger or other object near the eyes and concentrating the attention upon it for the purpose of optical convergence.

It consists of a flat base-board about nine by eleven inches, with an upright rod at one end bearing two sliding sockets to be clamped at any elevation like those of a retort stand, or adjustible by stiff sliding springs. The upper socket supports horizontally a small vane or card, having a slit or sight-hole one-fourth of an inch wide and three inches long from right to left. This slit has its middle directly over the centre of the base-board, and is intended to have the eyes directly over it, one eye at one end and the other at the other; two small holes, say one-fourth of an inch, occupying the place of the ends of this slit would answer, except for the unequal distances between the eyes of different observers. The lower of the two sockets bears horizontally a moveable screen of paste-board or thin wood, having a slit at least three inches wide from left to right, and about one inch in the other direction, with its centre also perpendicular over the centre of the base-board. This screen has marked vertically across its middle an index, shown by two arrows in the figures marked I.

Experiments.—In experimenting with the phantascope, the operator places whatever is to be tried upon the lower tabular base-board, looks downward through the upper slit and slides the screen up or down until he attains the adjustment required.

Exp. 1. Let there be two identical letters, say A, placed or drawn on the base-board about two and a half inches apart from left to right, let the moveable screen be nearly down, and directing the eye, not to the letters, but to the index, draw the sliding-screen bearing the index gradually upward; the two letters seen indirectly will appear as four, or each letter will be double AA   AA; continuing to raise the screen and to regard the index, the double images will recede more and more until their position will be thus:

A   A A   A;

continuing still to raise the screen, the two internal images approach until they are optically superimposed and coalesce into one, thus: A   A   A

This middle or superimposed figure is the phantom or image where there is really no object. Cease to look at the index I, and turn the attention to the base-board itself, and this phantom figure instantly vanishes. If the two letters be placed on the base-board at the same distance as the eyes are apart, say two and a half inches, then this normal position of the screen will be just half-way between the eyes and the base-board. If they are placed further apart, the screen must be raised higher; the distance from the eyes to the index-screen

being in all cases, to the distance from that screen to the base-board, as the distance between the eyes is to the distance between the objects viewed. In the case above, the phantom image is formed exactly as if there were a letter in the area of the index-screen of half the size of the primitive letters on the base-board, and optically the letter should appear then; but the knowledge of the observer that there is nothing at that place will often prevent the deception.

Exp. 2. Lay upon the base-board a card having letters or other figures which are identical in size and form, set in regular rows and at equal distances all over, thus:

A A A A A A

A A A A A A

A A A A A A

and proceed to raise the screen as before; you will form phantom images as before between each of these figures, or possibly you will superimpose the first object upon the third, when you will have, not a single phantom, but a whole plane of them, each pair presenting a phantom between. This phantom surface will be likely to effect a complete deception, and will rise from the base-board and coincide with the index plane, when it may be contemplated with the same deliberation and ease to the eyes as if it were a real object. This would be sure to be the case if the index plane were figured over in the same manner, but with figures properly reduced in size.

Exp. 3. Place two identical pictures of the same flower on the base-board, say they are an inch in diameter, and two and a half inches apart; place also on the edge of the index area a picture of a small flower-pot or vase, with flower stems as an index; then form the phantom image as before, and the flowers will appear in the vase so long as you contemplate the stems at the index-screen; but the moment the eyes are directed to the flowers themselves, the phantom vanishes.

Exp. 4. Let one of the above flowers be red and the other blue; the phantom will be purple. Sometimes, however, it will appear nearly red, and then again blue. This and some other experiments convince me that the attention of the mind, even when we are looking with both eyes, is often directed exclusively to the image in one eye; and perhaps after that tires, the image in the other is contemplated. If this be true, then the two eyes serve in the first place to fix the distance of an object by the amount of convergence, and in the next place to relieve each other by turns.

Exp. 5. Let there be a horizontal heavy line placed to the left and a vertical one to the right on the base-board, thus: — |, then adjust the screen and superimpose the images to form a phantom. That phantom will be a cross, and the whole will appear thus: — + |

Exp. 6. Do the same with any other parts of a figure of which one shall be the complement of the other; the phantom will be the complete figure. Thus, take the picture of a person, cut it out of the paper, and cutting off the head, place the body on one side of the base-board and the head upon the other; the converged phantom will be the complete figure, the head coming in from one side and the body from the other. It is perhaps unnecessary to say, that each part must be placed in its true elevation, though displaced horizontally. It was in repeating this experiment that I discovered that my eyes did not appear to be mates; for I saw the body clearly, but the head obscurely. After a little time, however, these conditions interchanged, and I saw the head clearly and the body obscurely. Nor did I seem to have any voluntary control over these conditions, but my eyes continued to relieve guard according to some rule of their own. This is rather an amusing experiment. The figure being beheaded, the phantom ghost appears between the two parts of the body; and from a little unsteadiness of the optical convergence, the ghost's head is inclined to attitudinize, and will sometimes start off a little from the body, and in returning will go a little too far, and will break the neck in the opposite direction. If the head of the experimeter be a little inclined, then the head of the phantom will come on too high or too low.

Exp. 7. I placed a card, having two perpendicular parallel lines, about two inches in length and three inches apart. On converging them in an attempt at superposition, I found the converged lines were not parallel, but came in contact at the upper end first, and diverged a little downward. Standing with my head erect, I repeated this experiment by converging voluntarily, and without the aid of any index, the parallel sides of a window; the same want of parallelism was exhibited; but on throwing my head backward and looking horizontally over my cheeks, the converged perpendiculars coincided throughout. I learned by this that both of my eyes do not rotate in one and the same horizontal plane. I got another person to repeat the same experiment; and he found the error of his eyes to be in the opposite direction, the converged perpendiculars meeting first at the bottoms. This proves a moral adage to be physically true, "we don't all see alike."

This instrument, and the researches into binocular vision,

serve to extend considerably our knowledge of the anatomy and physiology of vision, nor is the subject by any means exhausted. I have not time to investigate the matter fully, and shall be happy to see fair and honourable competitors enter the field. The verifications and variations of the experiments by Dr. Lathrop were gratifying to me.

This apparatus will illustrate many important points in optics, and especially the physiological point of "single vision by two eyes." It shows also that we do not see an *object* in itself; but the mind contemplates an image on the retina, and always associates an object of such a figure, attitude, distance and colour, as will produce that image by rectilinear pencils of light. If this image on the retina can be produced without the object, as in the phantascope, then there is a perfect optical illusion, and an object is seen where it is not. Nay, more, the mind does not contemplate a mere luminous image, but that image produces an unknown physiological impression on the brain. It follows, that if the nerves can, by disease or by the force of imagination, take on this action, a palpable impression is made without either object or picture. As this would be most likely to occur when actual objects are excluded, as in the night, we have an explanation of the scenery of dreams, and the occasional "apparitions" to waking persons. The murderer, too, has a picture stamped on the sensorium by the sight of his victim, which ever wakes into vibration when actual pictures are excluded by darkness.

LIX. *Preliminary Report on the Observations of the Aurora Borealis, made by the Non-commissioned Officers of the Royal Artillery, at the various Guard-rooms in Canada. By* Captain Lefroy, *R.A., F.R.S.*

My dear Sir, Woolwich, May 18, 1850.

If you think the accompanying Report will be interesting to the readers of the Philosophical Magazine, it is much at your service.

Yours truly,

R. Taylor, Esq. Edward Sabine.

The system of observations on the aurora borealis, permitted by Colonel Dynely, C.B., at my request, to be made at all our regimental guard-rooms, under the sanction of the officers in command, has now been continued for two years in Canada, and for one year in Nova Scotia and Newfound-

land. I have therefore pleasure in communicating to the officers and non-commissioned officers who have interested themselves in the subject, a short account of what has been done, for the sake of the encouragement which the results afford for persevering in the undertaking.

The printed instructions, dated 11th October 1848, expressed in a few words the objects in view in keeping these registers. They were—1. To ensure the observation of every aurora which should be visible in Canada, so as to afford a better criterion of the actual frequency of the phænomenon than can be given by observations at any one station. 2. To supply the means of judging how far variations of the magnetical elements, shown by the instruments at Toronto during cloudy weather, might be connected with aurora visible elsewhere. 3. To furnish data for computing the height or distance of the luminous region from the earth. 4. Lastly, to throw some light on the question whether or no the same aurora is not sometimes seen under considerably different forms by observers stationed not very far asunder.

It is not worth while to enter into some of these inquiries until all the materials for comparison are accessible, including the observations made in the United States under the instructions of the Smithsonian Institution, and those published in the Regent's reports. I shall confine myself, therefore, at present principally to the first of them.

In the year 1848, aurora, or auroral light, was observed at Toronto on 69 nights, although for the last six months of the year no observation was made after midnight. This number is exclusive of five observations of a luminous appearance in the clouds, referred to aurora, but not perfectly determined. Observations are to be found at other of our stations on many of the same, and on 57 other nights, exclusive of one doubtful one; making a total of 126 decided, and 6 doubtful appearances in Canada*. There are about 46 nights in the year on which it was clouded at all the stations; if we omit these, the proportion is 10 observations to every 26 nights on which observation is not wholly precluded by the state of the sky, or 39 per cent.

This proportion is greater than that given by any one station taken singly. We have—

At Quebec, in 1848,	52 obs.	to	188	practicable nights—	28	per cent.
At Montreal,	41	...	201	...	20	...
At Kingston,	64	...	218	...	29	...
At Toronto,	69	...	207	...	33	...
At London, C.W.,	33	...	178	...	19	...

* Including Newfoundland for November and part of December.

It is, however, probably less than the truth, as far as it expresses the actual frequency of the phænomenon, as I have considered observation to have been possible whenever nothing to the contrary is stated, which is most likely more than the facts would warrant; moreover, when we consider the short duration of some of the displays, and how close to the horizon others of them occur, it is difficult to believe that we have noted every one, even on nights when the sky was clear; it is probably set down as clear in many instances when it was sufficiently clouded near the northern horizon to prevent a feeble display from appearing. The dates included in the list at which it was seen at all the stations, which extend along a line of 500 miles, are Jan. 11, 16; Feb. 21, 23, 24; March 16, 24; April 1, 2, 5, 6, 7, 29; Aug. 21; Nov. 16. On several other occasions it was seen at every station at which the state of the sky permitted it; but there are one or two instances of clear sky at stations not recording aurora which was seen elsewhere.

Aurora, or auroral light, was observed at Toronto in 1849 on 63 nights, exclusive of 5 entries of an uncertain character, the observations terminating at midnight throughout the year. The other stations, including Newfoundland and Halifax, add 70 more, exclusive of 2 doubtful ones; making a total of 133 certain, and 6 uncertain appearances, in Canada, Nova Scotia, and Newfoundland*. The area included this year, measured from London, C.W., to Newfoundland, extends about 1150 miles from east to west; and measured from Quebec to Halifax about 140 miles from north to south. Owing to this great extent, there are but few nights (24) clouded at all the stations; and omitting these, the proportion is 39 per cent., or exactly the same as before. We have at—

Newfoundland, in 13 months, or from Nov. 26, 1848, to Dec. 31, 1849	59 obs.	to	178 practicable nights,		or	33 p. c.
Halifax, in 10 months, or from Jan. 14 to Oct. 31, 1849...	30	...	136	...		22 ...
Quebec, in 12 months of 1849	44	...	182	...		24 ...
Montreal	26	...	Descriptions imperfect.			
Kingston	34	...	178	...		19 ...
Toronto...........................	63	...	199	...		31 ...
London...........................	26	...	172	...		15 ...

In this list there are but two auroras seen at all the stations without exception; they occurred on Feb. 27 and July 23. There are eleven dates on which it was seen at Newfoundland and London or Toronto, but missed at some of the interme-

* One of the uncertain appearances at Toronto is confirmed by other observations.

diate stations. These dates are Jan. 14, Feb. 19, March 17, April 24, July 31, Aug. 12, Sept. 12 and 18, Oct. 7 and 30, Nov. 28. None of the stations singly give quite so many appearances as the previous year. The five Canadian stations, which gave 121 appearances in 1848, give but 99 in 1849, the remainder being made up from the other commands.

These observations having been continued throughout the night, may be referred to for testing an apparent law which was noticed in the observations made by Serjeant Henry and myself at Lake Athabasca in the winter of 1843-44, and which is fully confirmed by the series at Toronto, namely, that the aurora borealis does not appear with equal frequency at all the hours of darkness, but is subject, like most other phænomena in meteorology, to influences having a diurnal period as well as an annual one. The present series places the hour of maximum frequency at 10 or 11 P.M.; probably a longer continuance will be necessary to fix it accurately.

Table showing the number of the times on which Aurora is reported at each hour of the night.

Station.	5.	6.	7.	8.	9.	10.	11.	Mid.	1.	2.	3.	4.	5.	6.
1848.														
Quebec..............	2	5	16	23	25	31	32	23	20	16	17	12	7	1
Montreal	...	1	3	11	18	22	23	21	18	17	15	7	2	
Kingston	...	4	5	17	26	30	32	32	24	22	12	9	2	
London	...	1	6	12	18	17	14	11	9	6	7	6	3	
	2	11	30	69	87	100	111	87	71	61	51	34	14	1
1849.														
Newfoundland......	1	8	15	19	21	32	34	27	20	18	11	6	6	1
Quebec..............	...	2	5	12	11	20	15	12	9	5	4	2	3	
Montreal	...	3	3	5	4	8	11	11	11	9	6	4		
Kingston	...	1	6	10	11	14	9	8	6	5	6	3	1	
London	...	...	1	9	15	17	14	9	11	4	3			
	1	14	30	55	62	91	83	67	57	41	30	15	10	1
Two years	3	25	60	124	149	191	194	154	128	102	81	49	24	2

Any observation before 6 P.M. is here set down at 5, and so on.

The aurora appears in Canada in every month of the year. The greatest number of observations is in April; and there is a very marked excess in February, March, and April of each year over any other period. Taking them by the seasons, there are in the—

Spring, March, April, May,	1848	40	1849	41
Summer, June, July, August,	...	21	...	29
Autumn, September, October, November,	...	31	...	34
Winter, December, January, February,	1848-49	37	1849-50 (about)	20.

I believe that this number of observations is greater than has ever before been made in so low a latitude, and am inclined to think that it is very high even for Canada. The greatest number of observations at Toronto in any previous year since 1840 was 37 in 1846, the average of the ten years being 35. The greatest number in any one year (from 1837 to 1848) collected in the Regent's reports is 75, the average 50. The greatest number observed by M. Hansteen at Christiania, in Norway, lat. 60° from 1837 to 1846, in any one year, is 52—the average 33. This result is not more than may have been expected from the great advantages afforded by the duties of non-commissioned officers on guard for observations of the kind, and from our comparative proximity in geographical position to the magnetic pole, with which, in some way not at present well understood, the phænomenon appears to be connected. But it is highly satisfactory to find that the pains taken have been so successful. For the next twelve months' observations will be continued at Toronto throughout the night, and the observatory will be provided with a number of self-registering instruments, recording every change of the magnetic elements mechanically. Hence it will be of great consequence not to lose the key which auroral displays at a distance may possibly afford to those movements in a single instance.

At some of the stations the non-commissioned officers have got out of the habit of attempting to describe what they see. This is to be regretted. Measurements with the wooden quadrant, or careful estimations of the heights and azimuths of arches, are frequently wanting, and the time is not always stated. This remark applies particularly to the termination of the displays, which are frequently said in general terms to have lasted until daybreak; in all such cases the observer should state, as nearly as he can, the latest moment at which he was sure of seeing the light, watch its extinction attentively, and endeavour to decide for himself whether that is the consequence of the increase of daylight, or of the actual termination of the phænomenon. Very early appearances should, for similar reasons, be particularly described; for instance, it is recorded to have been seen at London, C.W., on the 24th of July 1848, at half-past 7 P.M., which is but a few minutes after sunset. Such a rare observation should have every possible confirmation. These particulars might at least be noted with very little trouble at the hours of going rounds. I should be glad also to see a more explicit statement every morning of what the character of the night has been, as regards the possibility of observing aurora, so as to give some

precision to the rough calculation attempted above of the percentage of nights in which it is seen, to nights in which it could be seen if it occurred. The expression "fine night" is ambiguous. In any statement of this kind, the point to be chiefly referred to is the condition of the northern half of the sky—the rest is of little consequence. I should be obliged by a memorandum on the next register of the nature of the look-out at each station, and how nearly down to the horizon the view from N.E. to N.W. extends; some difference in this respect is perhaps the reason why the observations are more numerous at some stations than at others.

Dates of all the observations included in the foregoing comparison. The stations are expressed by their initials:—N., Newfoundland; Q., Quebec; M., Montreal; H., Halifax; K., Kingston; T., Toronto; L., London, C.W.; P., Penetanguishene; F., Fenelon Falls; B., Bruce Mines, Lake Huron. The last three are additional stations, from which I have been favoured with communications:—

1848.

January.

3. K. T.
9. L.
11. K. T. L.
15. K. L.
16. K. T. L.
23. K.
28. M. T. L.
29. K.

February.

6. Q. M. K.
7. Q. M. K. T.
8. Q. M. K. T.
12. M.
13. M.
14. M. T.
20. L.
21. Q. M. K. T. L.
22. T.
23. Q. M. K. T. L.
24. Q. M. K. T. L.
25. Q. M.
28. Q.
29. K.

March.

1. M. T.
6. M. K. T.
8. T. L.

March (continued).

10. K.
14. Q. M. T.
16. Q. M. K. T.
19. K. T.
20. Q.
23. Q.
24. Q. M. K. T.
27. T.
30. M. K.
31. T.?

April.

1. Q. M. K. T. L.
2. Q. M. K. T. L.
3. Q.
4. K. T.
5. Q. M. K. T. L.
6. Q. M. K. T. L.
7. Q. M. K. T. L.
9. M. K.
15. M. K. T.
16. K.
17. Q.
20. Q.
21. K; H. T.
23. T. L.
24. T.
26. Q.?
29. Q. M. K. T. L.
30. Q. M. K. T.

May.

2. T.
4. Q.
7. M. K. T.
8. Q. T.
17. Q. T.
18. Q. K.
22. K.
24. K. T. L.
25. K. T.
26. T.
31. K. T. L.

June.

3. M. K.
5. K.
9. M.
22. T.
28. L.
29. Q. K. T.

July.

3. K. T. L.
4. K. T.
5. K.
10. Q. M.
11. K. T.
24. L.
27. L.
28. K.
29. K.

1848 (*continued*).

August.

1. M. K.
3. M.
8. Q. M. K. T.
21. Q. M. K. T. L.
22. Q. M. L.
28. T.

September.

3. Q. K.
4. Q. M. K. T.
17. K.
18. Q. K.
20. T.
29. T.
30. T.

October.

2. Q.
4. Q.
8. Q. M. K. T.

October (continued).

17. T. ?
18. T.
19. L.
22. K. T. L.
23. M. K. T.
24. T.
25. Q. T.
27. Q.
28. Q.
29. K.
30. Q.
31. K. T.

November.

10. T. ?
15. T. ?
16. T.
17. Q. M. K. T. L. P.
18. Q. T.
19. Q. T.

November (continued).

21. T. L.
22. Q. T.
23. Q. K. T. P.
25. T. ?
26. N. Q. K. T. L.
27. N. T.
30. M.

December.

2. N.
8. K.
13. N.
14. N.
17. Q. K. T. L.
18. N. K. T. L.
19. Q. L.
21. N.
22. K.
23. N. P.
25. N.
26. N. Q. M. K.

1849.

January.

7. N. Q.
11. Q.
14. N. Q. K. T. H.
17. P.
22. T.
23. N.
25. N.
26. T.

February.

3. Q.
9. Q.
12. Q. M.
13. Q. H. T.
14. T.
15. N. Q. M. P.
16. N. Q. T. L.
17. P.
18. Q. K. T. L. P.
19. N. Q. H. T.
20. N. Q. H.
21. Q. H.
22. N.
23. N.
25. Q.
26. N. K. T. P.
27. N.Q.M.K.T.L.H.P.

March.

6. Q.
9. L.
15. K.
17. N. Q. K. T. L. H. B.
18. Q. M. K. T. L. B.
21. T.
25. T. H.
26. T. L.
27. T.
30. N. K. T. L.
31. T.

April.

1. T. H.
2. Q. M. L.
4. H.
11. K.
13. M. T. H.
14. Q.
15. N.
16. T. L. H.
17. N. Q. M. H.
18. N. Q. M.
20. N. T. H.
21. Q. H.
22. N. T.
24. N. M. K. L.

April (continued).

25. N.
26. N.
27. N. Q.
28. L.
29. Q. H. ?
30. N.

May.

1. Q. H.
6. H.
11. N. H.
14. T. L.
17. T. H.
20. T.
21. T.
23. T. ?
25. K. T.
26. K.
27. M.

June.

6. Q.
8. K.
14. Q.
16. Q. ?
17. M.
18. M.

1849 (*continued*).

June (continued).

20. N. Q. T. L.
22. T.
24. K. L.
25. H. ?
27. Q.

July.

3. H.
4. H.
5. T.
9. N. T.
10. T.
12. L.
20. T.
21. N.
22. Q. T.
23. N.Q.M.K.T.H.L.
26. T.
31. N. Q. M. K. T. L.

August.

2. T.
4. N. K.
12. N. T. F. L.
13. N. M. K.
18. T. L.

August (continued).

20. N.
21. K. T.
22. Q. H.

September.

3. Q.
7. N. T.
8. K. T. L.
9. M. F.
12. N. K. T. H.
16. N. M.
17. T. F.
18. N. Q. M. K. T. H. F.
19. Q. M. H. F.
21. H.
24. K. T.
29. N. Q. M. K.

October.

7. N. Q. M. K. T. L.
9. M.
10. T.
13. Q. K. T. L.
14. Q. M. T. H. L.
17. N. K.
18. K. T. H. L.

October (continued).

19. K. H.
20. K.
21. N.
24. M. T.
30. N. Q. T. ? L.

November.

10. N.
12. T. K.
13. T.
14. K. T.
15. F.
18. K. T.
19. T. ?
21. T. ?
25. T. ?
26. Q. T.
27. K.
28. N. Q. M. T.
29. N.

December.

11. M. T.
12. N. T.
18. T.
20. N.

I will only add, that these observations promise to furnish a valuable body of information respecting the aurora, and will have a very important bearing on the observations to which the establishment at Toronto is devoted. While, therefore, I take this occasion of conveying my thanks to my brother officers, and to the non-commissioned officers of the regiment, for their assistance, I beg to renew my request that the system be persevered in.

J. H. Lefroy.

Magnetical Observatory,
Toronto, 27th March 1850.

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*Instructions of Observations of the Aurora**.

Non-commissioned officers on guard have an opportunity of observing at every second hour whether any aurora is visible; and by encouraging the more intelligent of the men, when their posts are favourably situated for the purpose, to notice and to report any display of short duration which may

* The original instructions are here extended and improved by the incorporation of some particulars from those issued by the Smithsonian Institute.

occur in the intervals, will be able to state every morning whether aurora has been seen at all during the night, and if not, whether the state of the sky was favourable or otherwise to observation. Private observers should make a regular practice of looking for auroras, every clear evening, from dusk to as late an hour as may be convenient, recording the result whether there has been an aurora or not, together with the times of observation. The notes may be short, but they should be clear and precise. Wet or cloudy evenings should be noted.

Auroral phænomena may be divided into the following classes:—

1. A faint light in the north, without definite form or boundary.

2. "A diffused light, defined by an arch below."

3. Arches resembling the rainbow in size and form, but of a uniform white colour, sometimes retaining their apparent position for a considerable time without change.

4. "A dark segment under the arch;" if any star can be distinguished within this space, the circumstance should be particularly noted.

5. "Floating patches of luminous haze or cloud."

6. Beams, rays, streamers, transverse and serpentine bands, sometimes tinged with colour, and undergoing more or less rapid changes. It may be necessary to define the last two expressions. Transverse bands are frequently nothing more than arches which have advanced nearly to the zenith, or perhaps have passed it, and retain their regularity of form, although now projected nearly as straight lines. Serpentine bands rather resemble curtains of light, and undergo in their outline changes like those of the folds of a curtain. They are usually the most brilliant part of a display.

7. "Auroral corona, or a union of beams a few degrees to the south of the zenith."

8. "A sudden appearance of dark clouds" in the region recently occupied by the aurora.

9. "Sudden appearance of haze over the whole face of the sky."

10. Lastly, a disposition in light clouds at a great elevation to arrange themselves during daylight in parallel lines, crossing the meridian at right angles, has been frequently suspected to be connected with the aurora, or with a common source.

The observer should state in plain and definite language the general character of the aurora, with reference more particularly to the foregoing characteristics. At Canadian stations every observation of the azimuths of the extremites of

an arch, when they are well defined, its span along the horizon, its height above it, or its place among the stars, will be valuable for comparison. At all stations the time at which the light passes to the south of the zenith should if possible be stated, as well as the precise times of very brilliant or active displays, which frequently last but a few minutes. Lastly, it should be noted how much beyond the zenith, to the south, the bands of light descend. The degree of brilliancy may be denoted by the terms—faint, moderate, bright, very bright.

LX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 141.]

Jan. 10, 1850.—"EXPERIMENTS and Observations upon the Properties of Light." By Lord Brougham, F.R.S. &c.

The author states that the optical inquiries of which he here gives an account were conducted in the first instance under the most favourable circumstances, arising from the climate of Provence, where they were commenced, being peculiarly adapted to such studies: he further states that he subsequently had the great benefit of a most excellent set of instruments made by M. Soleil of Paris; remarking, however, that this delicate apparatus is only required for experiments of a kind to depend upon nice measurements, and that all the principles which he has to note in this paper as the result of his experiments can be made with the most simple apparatus and without any difficulty or expense. His statement of the results of his experiments is thrown into the form of definitions and propositions, for the purpose of making it shorter and more distinct, and of subjecting his doctrines to a fuller scrutiny. He premises that he purposely avoids all arguments and suggestions upon the two rival theories, the Newtonian or Atomic, and the Undulatory.

The following are the author's Definitions and Propositions.

Definitions.

1. *Flexion* is the bending of the rays of light out of their course in passing near bodies.
2. Flexion is of two kinds—*inflexion*, or the bending towards the body; *deflexion*, or the bending from the body.
3. *Flexibility*, *deflexibility*, *inflexibility* express the disposition of the homogeneous or colour-making rays to be bent, deflected, inflected by bodies near which they pass.

Proposition I.

The flexion of any pencil or beam, whether of white or of homogeneous light, is in some constant proportion to the breadth of the

coloured fringes formed by the rays after passing by the bending body. Those fringes are not three, but a very great number, continually decreasing as they recede from the bending body, in deflexion, where only one bending body is acting; and they are real images of the luminous body by whose light they are formed.

PROPOSITION II.

The rays of light when inflected by bodies near which they pass are thrown into a condition or state which disposes them to be on one side more easily deflected than they were before the first flexion; and disposes them on the other side to be less easily deflected: and when deflected by bodies they are thrown into a condition or state which disposes them to be more easily inflected, and on the other side to be less easily inflected than they were before the first flexion.

PROPOSITION III.

The disposition communicated to the rays by the flexion is alternative; and after inflexion they cannot be again inflected on either side; nor after deflexion can they be deflected. But they may be deflected after inflexion, and inflected after deflexion, by acting on the sides disposed, and not by acting upon the sides polarized.

PROPOSITION IV.

The disposition impressed upon the rays, whether to be easily deflected or easily inflected, is strongest nearest the first bending body, and decreases as the distance increases.

PROPOSITION V.

The fringes made by the second body acting upon the rays deflected by the first, must, according to the calculus applied to the case, be broader than those made by the second body deflecting those rays inflected by the first.

PROPOSITION VI.

When one body only acts upon the rays, it must, by deflexion, form them into fringes or images decreasing as the distance from the bending body increases. But when the rays deflected and disposed by one body are afterwards inflected by a second body, the fringes will increase as they recede from the direct rays. Also when the fringes made by the inflexion of one body, and which increase with the distance from the direct rays, are deflected by a second body, the effect of the disposition and of the distances is such as to correct the effect of the first flexion, and the fringes by deflexion of the second body are made to decrease as they recede from the direct rays.

PROPOSITION VII.

It is proved by experiment that the inflexion of the second body makes broader fringes or images than its deflexion, after the deflexion and inflexion of the first body respectively; and also that

the deflexion fringes decrease, and the inflexion fringes increase with the distance from the direct rays.

Proposition VIII.

The joint action of two bodies situated similarly with respect to the rays which pass between them so near as to be affected by both bodies, must, whatever be the law of their action, provided it be inversely as some power of the distance, produce fringes or images which increase with the distance from the direct rays.

Proposition IX.

It is proved by experiment that the fringes or images increase as the distance increases from the direct rays.

These propositions are illustrated by particular instances, and their truth is shown by experiments and by some mathematical investigations. The author concludes his paper by a few observations tending further to illustrate and confirm the foregoing propositions, and for the purpose of removing one or two difficulties which had occurred to others until they were met by facts, and also of showing the tendency of the results at which he had arrived.

2. "Electro-Physiological Researches."—7th Series. By Signor C. Matteucci. Communicated by W. R. Grove, Esq., F.R.S.

In this memoir, Prof. Matteucci, after recapitulating the results of his previous researches on electro-physiology, published in the Philosophical Transactions, proceeds to the relation of new experiments. He first shows that nervous filaments made to conduct an electric current in a liquid are not capable, like metallic wires, of acting as electroids, and giving rise to electro-chemical decomposition. The solution employed was that of iodide of potassium; the nerves, two large ones taken from a living animal, each of which was separately attached to the metallic extremities of a pile of fifteen couples. No trace of decomposition followed; and he concludes from hence, that the conductibility of nervous matter is due to the liquid part of the matter itself.

He then gives further experiments on the relative conductibility of muscles and nerves, with a view to ascertain whether, when a current was impelled through a mass of muscle, any part of the current might have passed through the nervous filaments spread through that muscle. For this purpose he inserted the nerve of a galvanoscopic frog into a hole made in a piece of dead muscle, through which he then passed a very powerful current: no contraction followed in the galvanoscopic frog. When muscles still retaining their irritability were substituted for the dead muscle, induced contractions occurred in the galvanoscopic frog during the passage of the current. He concludes that when the poles of a pile of twenty-five or thirty elements are applied to the surface of the muscles of a living animal, the phenomena produced by the passage of the current must depend either on the *direct* action of the current on the muscular fibre, or on the *indirect* action or *influence* of the electric current transmitted

by the muscular fibre to its own nervous filaments, or rather to the nervous force existing in those filaments.

Referring then to an experiment related in a preceding paper, in which the lower limbs of a frog, united to the spine only by the lumbar nerves, are placed astride two glasses containing water, with each foot immersed, and in which a current, after traversing the two limbs, and consequently the two nerves, in opposite directions, so modifies at length the excitability of the nerves, that, on opening the circuit, only the limb in which the current has been passing inversely contracts, he shows that if in this state what may be called the 'inverse' nerve be touched by a piece of muscle, although the circuit is continued, yet the limb contracts as though the circuit had been broken. In fact, the muscle, by its greater conductibility, becomes traversed by the current in place of the nerve. Again, if after the former part of the experiment has been performed, the portions of nerve which had hitherto been buried among the crural muscles be dissected out, it is easily seen that their excitability has not been affected like that of the lumbar nerves, because the current in place of traversing them has traversed only the crural muscles. The nerve has had its excitability modified in only that part of its course in which, being laid bare and isolated, it has necessarily conducted the current.

M. Du Bois Reymond (*Comptes Rendus*) has related an experiment seeming to lead to the inference that section of the spinal marrow increases the excitability of the lumbar nerves, at least during a certain period of time. In order to test the accuracy of this conclusion on so important a point, M. Matteucci institutes a number of very accurate experiments, in which he measures the excitability of the lumbar nerves after section of the spinal marrow, by means of the apparatus of Breguet, used and described by him in a former paper. His first results show that "the contraction excited in the muscles of a frog, of which the spinal marrow has been divided from twelve to eighteen hours, is *stronger* than that obtained under the same circumstances from the muscles of a frog just killed, without having been previously subjected to any injury to its nervous system." But subsequent experiments have satisfied him that this result depends not on the separation from the spinal marrow, but rather on the repose in which the muscle has been permitted to remain; for without division of the marrow, nearly the same force of contraction existed after the same interval of time. He finds indeed that the only alteration which the excitability of a nerve undergoes by separation from the nervous centres, consists in its being more readily exhausted under the action of stimulants, the longer the period that has elapsed since its detachment.

The author then proceeds to relate the nature of the strict analogy existing between electricity and nervous force. As electricity is developed under the influence of the nervous current in the organs of electrical fishes, so, as a converse of this phenomenon, electricity may develope the nervous force. After adverting to the well-known analogy subsisting in every particular between the phenomena of the electrical organ and those of muscles, he adverts to the old experiment of passing a current through the muscles of the thighs of

a living animal, the positive pole being placed now above, now below, so that it may be supposed that the current passes in the two cases in opposite directions as regards the nervous filaments distributed in the muscles. He then points out that the effects of a current directed downwards, in the direct course of the nerves, are a strong contraction of the muscle traversed, and also of the *muscles of the leg below*; while the effect of a current in the opposite, or inverse direction, is *pain*, together with contractions less violent and always confined to the muscles traversed. The *contractions* (especially of the parts below) indicate a current of nervous force propagated towards the muscles, while the *pain* indicates a current towards the nervous centre. Now, bearing in mind that it has been proved by direct experiments that an electric current traversing a muscle never quits the muscular fibre to enter the nervous filaments, it seems clear that the phenomena just spoken of are exclusively owing to the *influence* exerted by the electricity passing through the muscles on the nervous force contained in the nerves; and also that this nervous force acts peripherad or centrad according to the direction of the electric current which excites it. The great importance of the conclusions drawn from these experiments consists in this, that they lead to the same law which establishes the analogy between nervous force and the electrical discharge of fishes. The paper concludes with some further considerations intended to confirm this law.

ROYAL ASTRONOMICAL SOCIETY.

[Continued from p. 311.]

Jan. 11, 1850.—On the Past History of the Comet of Halley. By Mr. J. R. Hind.

It is well known that the periodicity of the remarkable comet which bears the name of Halley was inferred by that astronomer from a comparison of three sets of elements, calculated on observations of the comets of 1531, 1607 and 1682. When his table of cometary orbits was first published, Dr. Halley says he was content to hint at his conjectures respecting the identity of the comets of those years as having some degree of probability, and to advise posterity carefully to watch for its return about the year 1758. At a subsequent period, an examination of the catalogues of ancient comets showed that three others had preceded those already mentioned, "manifestly in the same order and at like intervals of time, viz. in the year 1305 about Easter; in the year 1380, the month unknown; and, lastly, in the month of June 1456;" and on making this discovery Dr. Halley says he became much more confirmed in his former opinion. Want of data, however, prevented his ascertaining beyond doubt that the comet of 1682 had appeared in any of the three years 1305, 1380 or 1456, and he had little or nothing but equality of intervals, on which to found his belief.

M. Pingré, after collecting together the immense mass of records from which his great work the *Cometographie* is constructed, was enabled to convert into a certainty Halley's supposition respecting the appearance of his comet in 1456. Two definite observations of

the position were found, the one at Vienna, mentioned in an Austrian chronicle by Ebendorffer, the other at Rome, preserved in a manuscript treatise on the comet of 1468, to which Pingré had access in the Bibliothèque du Roi at Paris. The comet was in perihelion on the 8th of June at 22^h, Julian style.

The preceding appearance was referred by Halley to 1380, but M. Laugier has shown that it took place in the autumn of 1378, under nearly the same circumstances as in 1835. The Chinese observations fix the time of passage through perihelion on November 8^d 18^h. (*Conn. des Temps*, 1846.)

In a paper on the history of the comet of Halley, published in the *Comptes Rendus* for 1846, July 27, M. Laugier has recognised this body in 760 and 451. The elements calculated by Burckhardt for the comet observed by the Chinese in 989 approached so close to that of Halley's comet, that some suspicion of their identity has been entertained. But I believe, with these exceptions, the returns which I am about to mention as either certain or probable have not been previously noticed. The valuable details existing in the annals of China, and but recently known in Europe, enable us to trace this famous comet with a high degree of probability to the year 11 before the Christian æra,—a most important circumstance, not only as regards the history of this particular comet, but as bearing on the constitution of these bodies in general. I shall merely state here the results to which I have been led by a close examination of the *Cometographies* and Chinese records, without extending this notice to an inconvenient length by the insertion of details. The data which I have had to work upon are found in the *Cometographies* of Pingré, Hevelius and Lubienietski, Ma-tuoan-lin's catalogue of comets and extraordinary stars, and the other Chinese authorities with which we are acquainted through the labours of M. Edouard Biot.

The comet of Halley having returned to its perihelion in 1378, we may expect to find some mention of it about the year 1301; and notwithstanding the anomalous character of the results obtained by MM. Burckhardt and Laugier for the first comet of that year, I am pretty well convinced it was no other than the comet of Halley. The Chinese account, which is tolerably definite, is exceedingly well represented by the elements of that body: assuming the perihelion passage to have occurred on October 22^d 16^h Greenwich time, the comet passed through Gemini, south of Ursa Major, and finally traversed Serpens and Ophiuchus, being lost in the twilight at the end of October or beginning of November. There is one European account, however, which is not so easily reconciled with this supposition. It is that of Friar Giles, whose description has led M. Laugier to an orbit differing considerably in the position of the line of nodes from that of Halley's comet. Now had this Friar Giles established for himself a reputation as an exact and consistent recorder of facts, we might justly entertain serious doubts as to the identity of the comet of 1301 with Halley's; but in his account of another comet (that of 1264) he contradicts himself so

manifestly in the same paragraph, that we may fairly question his ability to describe accurately what he saw. Even in the case before us there is some confusion, and M. Laugier has been obliged to admit an alteration, which produces a contradiction in Giles's account, in order to reconcile the main part of his description with the Chinese observations.

The question stands thus: Halley's comet should have appeared in all probability about 1301. We find in that year mention of a comet, whose apparent path, according to the Chinese historians, is well represented by the elements of Halley's comet: the only objection to the identity that can be advanced rests on an observation of latitude by an European astronomer, or rather astrologer, whose description of another comet has been shown to contain a manifest contradiction, and who in the very case before us has committed one pretty decided error. I am strongly inclined to recognise in the first comet of 1301 Halley's famous star.

It will readily be understood that I have not the slightest intention to undervalue M. Laugier's investigation on this comet: there can be no doubt, that if we admit the whole narration of Friar Giles, his orbit must be retained, and in that case we must conclude that the comet of Halley passed its perihelion about the year 1301 unobserved, or at least unrecorded.

The preceding return of the comet took place, I think, in 1223, in the month of July, shortly before the death of Philip Augustus, as French historians state. It was seen for eight days at the beginning of July in the evening twilight. The Chinese have no mention of this comet, and it unfortunately happens that European chronicles give very vague accounts of it, so that we cannot come to any definite conclusion respecting its identity with Halley's. All that can be said is, that the few particulars we possess agree perfectly well with the position of Halley's comet in the heavens when the perihelion occurs in July, and that the comets of the preceding and following year, observed in China, appear to have had very different elements. It is therefore most likely that our comet was in perihelion in July 1223.

In 1145 the return of the comet of Halley seems to me little less than a matter of certainty. On carrying back the elements to that epoch, and fixing the perihelion passage on April 19, the whole of the particulars left us by the European and Chinese authors are exactly represented. Its discovery in the morning twilight about April 15, its increasing brilliancy towards the end of the month, the disappearance about the first week in May, and rediscovery in the evening sky in the north-west on May 14, and gradual fading away in Hydra on the 9th of June, are fully explained, the positions agreeing perfectly.

The periods of revolution between 1145 and 1456 average $77\frac{1}{2}$ years, and if we reckon backwards a like interval from 1145 we arrive at the year 1067, about which epoch we may expect to recognise the comet again.

There is vague mention in several European chronicles of the ap-

pearance of a comet at the time of the death of Constantine Ducas, in May 1067. We have no particulars, and even the reality of the comet is subject to some doubt. In 1066, a year memorable in English history as that of the Norman Conquest, a very grand and remarkable comet is recorded by nearly every chronicler and historian of the age. It was observed throughout Europe and China during the months of April and May. The elements of Halley's comet, as they at present exist, will hardly represent these circumstances with a sufficient degree of accuracy; but if we assume the following numbers, we shall have a very fair agreement between observation and calculation:—

Perihelion passage 1066, April 1^d 0^h, Greenwich time, Julian style.

Longitude of perihelion	264° 55′
Ascending node	25 50
Inclination	17 0
Least distance	0 72

Motion retrograde.

In this orbit the longitude of perihelion is further in advance than that of the present orbit of Halley's comet by 30°, and the node by 17°: the perihelion distance is greater by 0·14. But are these differences to be considered beyond the limits of probability? In the lapse of so many centuries, may not the planetary perturbations have produced alterations in the elements at least equivalent to those here exhibited, especially since there is good reason to conclude that the plane of the orbit of Halley's comet formerly coincided much more closely with the plane of the ecliptic than at present, the comet being therefore subject to far larger perturbations than it has undergone in more recent times?

The elements which I have assumed for the comet of 1066 have been obtained partly from the observations in that year, and partly from the Chinese description of a comet A.D. 141, which also agrees with the supposed period of Halley's. *The same orbit will represent the apparent paths in both years*; and I would particularly insist on this point as one of some importance in the present inquiry. I may also remark, that it appears by no means improbable that an orbit more closely resembling that of Halley's comet might be made to represent the circumstances recorded of the comet of 1066 with tolerable accuracy. The descriptions of this object are so confused, that a good deal of uncertainty is necessarily attached to any conclusion we may deduce from the observations.

The preceding return of Halley's comet took place, I think, in 989, and Burckhardt's calculations relative to the comet observed in China in that year strongly support this idea. The perihelion passage would occur about the 12th of September.

In 912 a comet was seen in China, in the month of May, in Leo, near the star marked χ on our charts. It was also perceived in Europe. The orbit of Halley's comet, with very trifling alterations, will agree with the above position about May 13 or 14, if we fix the perihelion at the beginning of April.

The preceding appearance of the comet should fall about 837, in which year a most splendid comet was observed both in Europe and China. The elements of this body, calculated by Pingré, exhibit a general similarity to those of Halley's comet, with the exception of the node, which is almost diametrically opposite. Now Pingré conjectured, as he himself states, that the comet passed the *ascending*, and not the *descending* node on the 10th of April, or otherwise it could hardly be said to move in a north-west direction on the 12th, as we learn from the Chinese relation. It is clear, from the position of the tail on the former date, that the head of the comet could not have been far from the ecliptic ; and it appears to me that Pingré's conclusion is unavoidable, if we take into consideration the whole of the Chinese description, notwithstanding its laxity as regards the distances of the comet from the equator, and the singularity of the circumstance that a comet should have become visible in the very year when Halley's would probably return to the perihelion, presenting in every element except the node a striking resemblance to the orbit of that interesting body. The comet of 837, which figures in our catalogues, was therefore in all probability different from Halley's, though the position and distance in perihelion and direction of motion were the same, and the inclination of the orbit (a most uncertain element in the present case) not very widely different for the two bodies.

The Chinese annals mention another comet in May and June 837, which was probably that of Halley. It is not unlikely that successive copyists have altered the original description of the path amongst the stars, and that the comets observed in Gemini and Virgo in these months were the same. For the Chinese accounts of the apparent tracks of comets present frequent instances of want of chronological arrangement: thus a date is occasionally mentioned as the epoch of discovery, and a position corresponding to a subsequent time immediately follows. If we may interpret the Chinese description, so as to place the discovery of Halley's comet in Gemini on April 29, and to refer the positions in Leo and Virgo to that body at a subsequent date, it will be easy to reconcile the apparent path with calculation, supposing the perihelion passage to have occurred early in April.

If the comet of Gemini and Leo was not that of Halley, probably this object was missed altogether at this return.

M. Laugier has shown in the most satisfactory manner that the observations of the comet of 760 in Europe and China are perfectly represented by the orbit of Halley's comet, the perihelion falling on June 11. This year accords with my intervals. The return of the comet in 760 appears to me little short of a certainty. (*Comptes Rendus*, 1846, July 27.)

In September and October 684 a comet was seen in China in the western heavens; but no further particulars are given. If Halley's comet reached its point of least distance from the sun in October, it might have been observed in this position in September and the early part of October.

The previous return should have taken place about the year 607, and the Chinese annals have several comets in that year. I find, by actual computation, that none of them present any decided indications of identity with the one which forms the subject of these remarks, and I am therefore inclined to fix its reappearance in the following year, 608, when a comet is mentioned by Ma-tuoan-lin, though (most unfortunately) he has omitted to state the days to which his positions apply. The path attributed to this body, from Auriga, through the lower part of Ursa Major, into Scorpio, where the comet vanished, is precisely that which Halley's comet must follow when the perihelion takes place in October or early in November. This circumstance, and the close agreement of intervals, appear to render it highly probable that the Chinese observed the comet of Halley in 608.

After a careful examination of the particulars related of the comets of 530 or 531, which Newton and Halley, owing to the want of precise data, recognised as that of 1680, I find the whole of them may be explained by the elements of Halley's comet, supposing it to have been in perihelion early in November. Yet this inference is necessarily open to considerable doubt, and I am very far from insisting upon it. Of one point I have become pretty well convinced by my calculations, viz. that the comet of 530 or 531 (for the year of appearance is doubtful) was not identical with the celebrated one of 1680. Pingré seems to have suspected this, though he has endeavoured, by alteration of dates or positions, to show that such identity may have been possible. Where however we find the accounts of a comet as they stand in the original authorities reconcilable with a single orbit, it is surely unfair to alter them in any way, so as to produce an agreement with some preconceived notions.

In 451, or at an interval of about 79 years from 530, a comet was observed in Europe and China. It appeared about the time of the battle of Chalons, when Attila was defeated by the Roman General Aetius. On May 17 the Chinese saw it near the Pleiades, and followed it till July 13, when it was situate near β Leonis. Assuming Halley's comet to have arrived in perihelion on July 3 at midnight, M. Laugier finds a remarkable agreement between the observed and calculated positions, and there can remain but little doubt that this body was seen in 451.

Seventy-eight years previous, or in October 373, 24th day, the Chinese mention a comet in Ophiuchus and Serpens. Suppose our comet to have been at its least distance from the sun early in November, we shall find it would be located in Ophiuchus on the 24th of October. The account is too vague, however, to allow of any definite conclusion.

Deducting another period of 78 years, we arrive at the year 295, and at this epoch I find recorded a comet in Ma-tuoan-lin's catalogue which has every appearance of identity with Halley's. The path assigned by this historian is exactly represented by the orbit of Halley's comet, assumed to be in perihelion at the commencement of April. I passed through the lower part of Ursa Major, Leo and

Virgo, having been previously seen with the same right ascension as the constellation Andromeda. A comparison of the track in 295 with the paths followed by the comets in 451, 760 and 1456, will sufficiently justify the inference that the observations of the year 295 really belong to the comet of Halley.

Another interval of 77 years from this epoch brings us to the year 218, when this famous body should have visited us again; and it is an important fact that the Chinese annals mention a comet in 218, which there can scarcely be a doubt was the one in question. It was seen also in Europe shortly before the death of the Emperor Opilius Macrinus, who was killed on the 7th of June. Dion Cassius describes it as a very fearful star, and the Chinese tell us it was intensely brilliant. It passed through Auriga, Gemini, Ursa Major, into Leo, but was observed first in the morning in the eastern heavens. I fix the time of perihelion passage of Halley's comet on April 6, and find every circumstance recorded of the comet of 218 faithfully represented.

The preceding return fell, I think, in the year 141, and a fine comet is referred to that year by the Chinese historians. It was seen first in Aquarius and Pegasus, in the morning sky, about March 27, and about three weeks subsequently became visible in the evening, traversing Taurus, Gemini, Leo, &c. The elements of Halley's comet, unaltered, do not quite agree with this track and the dates attributed to the various positions; but if we suppose the following orbit, depending on the observations of 1066 and 141 (as already mentioned), we shall have a very fair agreement:—

Perihelion, March 29·1.

Longitude of perihelion	251° 55′	Equinox of 141.
Ascending node	12 50	Equinox of 141.
Inclination	17 0	
Least distance	0 72	

Motion retrograde.

These elements have great resemblance to those of Halley's comet, and it is very likely that an orbit differing still less might suffice to produce a tolerable agreement. But few comets are recorded about this year, and none of them exhibit any indications of identity with Halley's, except that of 141.

In the catalogue of Ma-tuoan-lin we find a comet in the year 65, and another in 66, either of which may possibly have been Halley's, though I think the latter agrees better on the whole. It was discovered in January in the eastern heavens; on February 20 it had the same right ascension as the star β in Capricornus, and advanced to the south of Scorpio. These circumstances are in perfect accordance with the track followed by Halley's comet when the perihelion passage takes place on January 26.

The few particulars we have respecting the comet of A.D. 65 may be represented by the orbit of Halley's comet, admitting it to have reached the perihelion on August 5. It is possible therefore that

the sword-shaped sign that was seen over Jerusalem at the commencement of the war which ended in the destruction of the Holy City by Titus may have been the comet of Halley.

The most ancient, and at the same time one of the most certain apparitions of this body, took place in the year 11 B.C., reckoning according to the manner of astronomers. It was observed, according to Dion Cassius, under the consulate of M. Messala Barbatus and P. Sulpicius Quirinus, before the death of Agrippa, and seemed as though it were suspended over the city of Rome. The Chinese found it on the 26th of August in Gemini; it passed over this constellation, north of Castor and Pollux, towards Leo and Virgo, moving at the rate of 6° daily. Subsequently it passed near Arcturus and other stars in Boötes, and arrived in Ophiuchus and Serpens. Fifty-six days after, August 26, it set with π and σ Scorpii.

After the publication of M. Biot's valuable details in the appendix to the *Connaissance des Temps* for 1846, I attempted an orbit for this comet, and was immediately struck with the similarity of the elements to those of the comet of Halley. The only alteration necessary appeared to be a diminution of the orbital inclination, which, instead of 17°, would be more satisfactory at 8° or 10°. The Chinese description cannot be strictly followed, or we should have a very irregular path; but I am satisfied that the elements of Halley's comet for perihelion, node and least distance, and an inclination of 8°, will accord as well with the observations as any orbit can possibly do.

Previous to the year 11 B.C. the accounts of comets become so vague that it would be vain to attempt to carry the inquiry into more remote antiquity. I think it will be deemed a fact of considerable interest that the celebrated comet which bears the name of our countryman Halley may be traced, in a pretty satisfactory manner, as far back as the year 11 before the Christian æra. For this extensive knowledge of its probable history we are mainly indebted to the records preserved in the annals of the various reigning dynasties in China.

LXI. *Intelligence and Miscellaneous Articles.*

ON BRONGNIARDITE, A NEW MINERAL. BY M. A. DAMOUR.

M. CASTELNAU, during his last journey in America, collected a considerable number of mineral substances, which he deposited with M. Damour to examine. Among these minerals, there was a large specimen possessing metallic lustre, and described as an ore of silver. As it did not possess any trace of crystallization, it was only by analysis that it could be ascertained to be a distinct species, consisting essentially of sulphur, antimony, lead and silver. Its principal characters are these:—It has the metallic lustre peculiar to the antimonio-sulphurets, such as polybasite, bournonite, zinkenite, &c. Its fracture is uneven, and it has no cleavage. Its powder blackish-

gray. It scratches calcspar, and is scratched by a steel point. Its density is 5·950. When heated on charcoal, it decrepitates, and quickly fuses at a temperature below incipient redness, emitting a sulphurous smell and white vapours. After long-continued roasting, it leaves a globule of silver surrounded by a yellow aureola, indicating the presence of oxide of lead. When heated in a close glass tube, it decrepitates, fuses, and yields a small orange-red sublimate, surmounted by a white sublimate. If heated in an open glass tube, it decrepitates, fuses, disengages a strong sulphurous odour, and a white sublimate of peroxide of antimony is deposited on the sides of the tube.

Concentrated nitric acid attacks it rapidly, with the evolution of nitrous vapours; the silver dissolves, and a residue is left, formed of sulphur, oxide of antimony and sulphate of lead. Nitric acid, diluted with four times its volume of water, attacks it slowly, evolving sulphuretted hydrogen; the silver and lead are partially dissolved; a gray deposit is left, in small needles, consisting of sulphuret and oxide of antimony, containing a considerable portion of lead and silver.

Hydrochloric acid, when concentrated and boiling, dissolves this substance completely, evolving sulphuretted hydrogen. On cooling, the solution becomes turbid, and deposits chloride of silver mixed with chloride of lead.

A boiling solution of caustic potash attacks this mineral when finely pulverized. It thus dissolves a large quantity of sulphuret of antimony; a black heavy powder remains, which consists of sulphuret of silver and lead containing some antimony.

If sulphur be added to the potash solution, the greater part of the antimony may be separated by successive decantations; the author, however, never succeeded by this method in separating the sulphurets of lead and silver perfectly from the sulphuret of antimony.

The analysis of the mineral was performed by acting upon it with a current of dry chlorine, in the manner described by M. Rose. The volatile chlorides of sulphur and antimony were separated by exposure to a gentle heat from the fixed chlorides of silver and lead. The sulphur was converted into sulphuric acid, and its quantity deduced from that of the sulphate of barytes which it yielded; the antimony was precipitated by sulphuretted hydrogen; the sulphuret obtained was analysed by the direct determination of the sulphur, and inferring the antimony.

The fixed chlorides were treated with boiling water, which dissolved the chloride of lead; the chloride of silver remained insoluble; its weight indicated the proportion of silver; the chloride of lead was converted into sulphate, and the weight of it gave that of the lead.

The liquor separated contains a minute quantity of copper, iron and zinc, the quantity of which was ascertained by the usual processes.

The mean of three analyses gave—

Sulphur	19·24
Antimony	29·77
Silver	24·77
Lead	24·91
Copper	00·62
Iron	00·26
Zinc	00·36
	99·93

The above results indicate the constitution of the mineral to be 5 equivs. sulphur, 2 equivs. antimony, 1 equiv. silver, 1 equiv. lead. In composition it approaches the *schilfglaserz,* described in Dufrenoy's Mineralogy, containing, according to Wöhler,—

Sulphur	18·74
Antimony	27·38
Silver	22·93
Lead	30·27
	99·32

Considerable difference in the composition of these two substances is however observable; the crystallographical characters of the schilfglaserz serve to distinguish one from the other; in fact, the latter mineral crystallizes in the system of the right rhombic prism, and gives well-marked cleavages; whilst the new mineral has a shining uneven fracture, without any indication of cleavage. It is to be expected, however, that it will at some time be found in regular crystals.

The specimen brought by M. Castelnau comes from the Mexican mines, and weighs about 14 lbs. One of its surfaces is sprinkled with iron pyrites. From its size and appearance it may be presumed to form a workable seam.

M. Damour proposes for this substance the name of *brongniardite,* as a tribute of respect to the memory of the late M. Brongniart.—*Annales des Mines,* vol. xvi. p. 227.

ACTION OF SULPHATE OF MAGNESIA AND SULPHATE OF ZINC.

M. Schaeuffele states, that when a saturated solution of sulphate of magnesia is put in contact with pulverized sulphate of zinc, the salt obtained differs from that procured by the inverse operation. In the first case the crystals contain 12·59 per cent. of magnesia and 11·60 of oxide of zinc, whilst in the second case the quantities are 27·84 of oxide of zinc and only 0·27 of magnesia.

The same fact is observed with regard to sulphate of iron in combining with sulphate of zinc. If the first of these salts be taken in powder, and the second in saturated solution, the crystals of the double salt obtained contain 13·80 per cent. of oxide of zinc and 12·10 of oxide of iron; whilst, on the contrary, if a saturated solution of sulphate of iron be taken, and the sulphate of zinc in powder, the double salt formed contains 14·63 of oxide of iron and 12·05 only of oxide of zinc.

The two salts, in combining, obey then some other law than that of their reciprocal affinity: this new influence M. Schaeuffele describes as the effect of masses.

In fact, it cannot be mistaken that the relative mass of the constituent salts has a peculiar action on the definitive product, since it is always the sulphate that has served to form the saturated solution which predominates in the double salt obtained, and it is also that which impresses its form on the product obtained.

A fact, which had long escaped the notice of chemists, is thus generalized, and to which M. Gerhardt first drew attention. What happens in the preceding experiments may be assimilated to that which is produced in the reaction of two solutions, according as one is poured into the other.—*Journ. de Pharm. et de Chim.*, Avril 1850.

ON ARSENIC IN THE ZINCS OF COMMERCE.

M. Schaeuffele observes, that it is not requisite to insist on the importance of this subject. As zinc is not only employed in manufactories for culinary purposes and in medico-legal investigations, it has become necessary to determine the relative proportions of arsenic which it may contain. The experiments of the author were made on four specimens of zinc of well-known origin; and he submitted them to the test of two methods generally applied in researches for arsenic, those of Villain and Jacquelain. The former consists in passing the arseniuretted hydrogen gas through a glass tube heated to redness, and in weighing the metallic arsenic resulting from its decomposition; the process of the latter consists in passing the gas through tubes with bulbs filled with solution of chloride of gold, which it decomposes, giving rise to metallic gold, hydrochloric and arsenious acids.

M. Villain's method gave the following results, as the quantity of arsenic contained in a kilogramme of each of the kinds:—

	gr.
Zinc of France	0·004260
Zinc of Silesia	0·000970
Zinc of Vieille Montagne......	0·000620
Zinc of Corphalie	0·000038

M. Schaeuffele also examined ten other commercial samples of zinc, but of unknown origin; and he found the quantities of arsenic comprised between the above-stated limits.

It results, therefore, from these experiments, that the zinc from the mine of Corphalie is the purest of those met with in commerce; the author indeed admits, that, on account of the small quantity of arsenic which it contains, it may be employed in medico-legal researches without previous purification, which cannot be done with the zinc of France without danger, it being the most arsenical of all. —*Journ. de Pharm. et de Chim.*, Avril 1850.

ON HUMUS. BY M. SOUBEIRAN.

It is well known that old trees are met with in forests the trunk of which decomposes slowly internally, and eventually becomes a brown-coloured powder of greater or less intensity. When this decomposition is much advanced, a moderately strong blow is sufficient to occasion this product of decomposition to fall in abundance. The author collected the altered wood which served for his experiments from an old oak, which had a large hole low down, level with the soil. It was moist; its colour that of Spanish snuff; its properties were those of the purest mould. It was insipid and inodorous; it yielded no colour to water, and gave a deep-coloured solution with ammonia; treated afterwards with an acid, and then again by ammonia, it was coloured. Lastly, this old wood, thus exhausted, soon again gave another coloured liquor by contact with the air and alkalies. The powder of the old wood therefore consisted of a mixture of pure humus, a little humate of lime, and a substance not yet altered, but capable of being changed into humus by contact with the air and alkalies.

The author extracted the humus from this old wood by washing it at first with water, and then treating it with ammonia. This solution was precipitated by hydrochloric acid, and the humus purified by repeated washings with water, boiling alcohol and æther. It was then analysed. It left 7·16 per cent. of ash. By Varrentrapp and Will's process, it yielded 2·5 per cent. of nitrogen. Deducting the ash, combustion by oxide of copper and chlorate of potash gave—

Carbon	55·0
Hydrogen	4·8
Nitrogen	2·5
Oxygen	37·7
	100·0

Subtracting the nitrogen, these numbers approximate $C^{34} H^{18} O^{18}$.

It is worthy of remark, that in this humus, formed by exposure to air and moisture for many years, the carbon did not exceed 55 per cent. It seems that this is the extreme limit of the decomposition which lignine can undergo unassisted by heat and the concentrated alkalies. This limit, it will be observed, is far removed from that assigned by M. Peligot to artificial ulmin, or 72 per cent.

As to the nitrogen, which is always one of the constituents of humus, it is impossible to say what proportion belongs to it, and what proportion enters into the nitrogenous products, which are mixed with it. It is to be observed, that, in the powder of oak used, the quantity of nitrogen is greater than in the wood which gave rise to it. This fact renders it probable that some of the nitrogen of the air is fixed in it during the decomposition of the wood; this was the opinion of Theodore de Saussure. It may be argued, that the remains of insects have supplied it; but for a long period this powder could have afforded them no shelter, since the slightest shock causes it to fall at the foot of the tree.—*Journn. de Pharm. et de Chim.*, Mai 1850.

ON A NEW REAGENT FOR ASCERTAINING THE PRESENCE OF SUGAR IN CERTAIN LIQUIDS. BY M. MAUMENÉ.

Chemists have indicated several processes for the detection of sugar, even under the singular circumstances of diabetes. Unfortunately no one of these processes is sufficiently simple for adoption in medical practice. The author proposes one by means of a reagent of tissue, which will instantly discover the presence of the smallest quantity of sugar.

The action of chlorine on sugar is very imperfectly known, and the experiments which M. Maumené has performed have shown numerous inaccuracies in the assertions which have been made by celebrated chemists. Thus chlorine acts even on dry sugar; it requires only 212° F. to excite the reaction; when cold, more time is required. In all cases a brown matter is formed, partly soluble in water, becoming a brilliant black caramel when it is dried. This is obtained by chlorine, and it is easily procured with the chlorides, and especially with the perchlorides.

All sugars act in the same way as cane-sugar with the chlorides; all undergo the dehydratation, the brown-black product of which is the final completion. And this is not all; as may be foreseen, substances, the composition of which is analogous to that of sugar, and which may also be represented by carbon and water, undergo the same kind of alteration: this is the case with lignin, hemp, flax, cotton, paper, starch, fecula, &c.

From all these facts results a knowledge of the conditions requisite for preparing a substance which has imbibed the reagent proper for discovering the presence of sugar. Strips of white merino answer this purpose; after having soaked it during three or four minutes in an aqueous solution of bichloride of tin, made with 100 parts of the bichloride of commerce and 200 parts of water, the merino is drained, and when dried in a water-bath on a strip of the same stuff, the reagent is prepared. It is to be cut into portions of 7 to 10 centimetres in length and 2 to 3 wide, like common test-papers.

By employing this chlorinated merino, the physician can, without any trouble, determine whether the urine contains an appreciable trace of sugar. It is sufficient to let fall a drop of the urine on the prepared test, and to hold it over red-hot charcoal, or the flame of a spirit-lamp, to produce in a minute a very visible black spot. The sensibility of this reagent is extreme; ten drops of diabetic urine, added to 100 cubic centimetres of water, form a liquid with which the test is rendered completely brownish-black. Common urine, urea and lithic acid, yield no such discoloration with chloride of tin.—*Comptes Rendus*, Mars 18, 1850.

FORMATION OF ASPARTIC ACID WITH BIMALATE OF AMMONIA. BY M. DESSAIGNES.

To M. Piria is due our knowledge of the interesting fact, that asparagin and aspartic acid, submitted to the oxidizing action of nitrous gas, disengage nitrogen and leave a residue of malic acid. He

has also shown, by the analytic method, that these two substances may be considered as amides of malic acid, corresponding to oxamide and oxamic acid. If this be the case, synthesis should reproduce asparagin and aspartic acid. The action of ammonia on malic æther, when this æther shall be prepared, ought to give rise to asparagin. The author states, however, that he has not been more fortunate than his predecessors in his attempts to obtain malic æther; but he has succeeded in preparing aspartic acid with bimalate of ammonia.

When this salt is heated from 160° to 200° (Centig.) in an oil-bath, it fuses and swells, and disengages some very slightly ammoniacal water. The residue is a reddish, transparent, resinous-looking mass, which is very slightly soluble even in boiling water. By repeated washings with hot water, a pulverulent, amorphous matter is obtained, which is of a pale-brick colour and an earthy taste. It is a new nitrogenized acid, which differs in all its reactions from aspartic acid. This substance is very stable. It dissolves in hot concentrated acids, from which water precipitates it unchanged, even after boiling for a short time. But if it be heated for five or six hours with nitric acid or with hydrochloric acid, it undergoes a remarkable transformation. The reaction is over when water, added to the acid solution, ceases to precipitate anything. The solution, evaporated in a water-bath to dryness, left a brown residue, crystalline and very acid, which is a compound of hydrochloric acid and organic matter. This compound is easily purified by charcoal, and fine colourless crystals are obtained. It was dissolved in a large quantity of hot water, and the solution was divided into two equal portions, one of which was accurately saturated with ammonia, and then added to the other portion. On cooling, a great quantity of small brilliant prisms were formed, which were aspartic acid; it did not possess the same form as the acid obtained from asparagin, but the salts which it forms with lime, soda, and with the oxides of copper and silver, crystallize in the same form as the corresponding aspartates; and the author ascertained by analysis that they contained the same quantity of base; the acid submitted to analysis gave the same results as those obtained from the compound of aspartic acid.—*Comptes Rendus*, Mars 18, 1850.

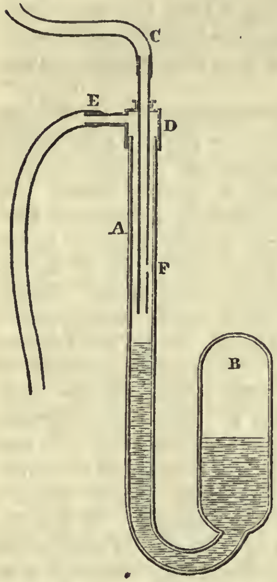
DESCRIPTION OF AN APPARATUS FOR REGULATING THE HEAT PRODUCED BY A GAS-BURNER. BY ALEXANDER KEMP.

Any one who has had occasion to maintain an object at a regulated temperature for a length of time by means of a gas-burner, must have experienced the difficulty of keeping the heat at the required point from two causes. First, the quantity of gas passing through the burner in any given time varies directly as the pressure on the service-pipes; a greater quantity is therefore consumed when the pressure increases, and the amount of heat applied to the object is increased in the same degree.

Secondly, as the temperature of the atmosphere is liable to change during the continuance of the experiments, its cooling influence will be greater at one time than another. Both these causes conduce to

render the attainment of the object in view one of considerable difficulty.

It will be evident, from what I have just stated, that any instrument intended to supply the desideratum must be self-acting, and must supply the gas to be consumed exactly as it is required. The instrument I have been in the habit of using for some time back consists of an air-thermometer, AB, containing mercury in the lower part of the bulb B and part of the stem A. A tube of smaller diameter, marked C, passes down the axis of the tube A, the annular space being made air-tight by a small brass stuffing-box, D, by means of which it may be retained at any required elevation. In using the instrument, the bulb B is placed in the same situation as the body to be exposed to the heat of the gas-flame; for instance, if it is a water-bath containing vessels or other objects, it is immersed in the water; or if an air-chamber or hot-press, it is placed as near the object as possible, so that the air in the bulb may attain the same temperature as the surrounding medium.



A tube of vulcanized India rubber is now to be connected with a stop-cock attached to the service-pipe, and its other extremity drawn over the end of the pipe C, which will make it sufficiently air-tight. A second India rubber tube is in like manner to be attached at E, to convey the gas to the burner employed as a source of heat in the operation.

Let us now suppose that it is required to keep an object at a temperature of 100° F., the bulb of the instrument being placed contiguous to the object; the stop-cock is to be opened so as to allow a free supply of gas to the burner, which is now to be kindled; the heat now begins to act on the air contained in the ball of the instrument, causing it to expand and force the mercury up the stem A; when it is found, by the use of a common thermometer, that the heat has risen to 100°, the tube C is to be pushed down till its lower extremity is immersed in the mercury. This would, of course, cause the flame to be extinguished; but, to prevent this occurring, a small hole is bored through the inner tube opposite F, which permits a small quantity of gas to pass to the burner. As the passage of the gas is now interrupted, the source of heat is withdrawn, and the cooling influence of the surrounding air comes into play, which will cause the air contained in B to contract and the mercury to sink in A, leaving the end of C uncovered, and thus open a free passage to the gas, which by its combustion would again raise the

temperature so as to cut off the supply; but in a very short time these two opposing forces come to an equilibrium, and the flame scarcely varies in size.

After trying the instrument in the form described, I experienced a practical difficulty from the want of perfect contact between the end of the tube C and the mercury, from which I found that it might rise many degrees beyond the assigned limit without sufficiently lowering the flame; this I at once saw might be overcome by making the tube of a substance which would become *wetted* by the mercury. I tried a brass and also a copper tube, amalgamated at the end, but they slowly dissolved in the mercury, which they rendered impure, so that its rise and fall could not be depended on with the required nicety. The substance now used is platinum, of which about half an inch of the lower end of the tube is formed, amalgamated by dipping it into a liquid amalgam of sodium and mercury. In connexion with this I may mention, that platinum, iron and steel may be readily amalgamated in the same way, or by using a strong solution of caustic potash or soda in contact with the mercury.

I have now used several of these instruments for various periods, and have found them to answer well. In one operation, conducted in Professor Gregory's laboratory (the fermentation of sugar into butyric acid), one of them has been in use for upwards of six weeks, keeping about five gallons of liquid at a temperature of 98° F., and it has not been observed to vary. I have also another in an apparatus for artificial incubation, where the temperature in the well part is kept at 120° F. For this latter purpose I consider it well-adapted, as it may be placed in the vessel along with the eggs, and thus the use of hot water be altogether dispensed with, as well as the constant attendance required to regulate the heat applied to the eggs.

I need not take up space with pointing out many applications that may be made of the instrument; these will occur to every one: but there is one I may allude to, viz. obtaining products of the decomposition of organic bodies at fixed temperatures, which has hitherto been somewhat difficult, as different substances are formed as the temperature varies to which they are subjected. The only limit to its application is the same as in the ordinary thermometer, viz. the boiling of the mercury; this might be overcome by using an easily fusible metal, such as tin, and constructing the instrument of iron, when its form might also be modified to suit particular circumstances.—*From the Chemical Gazette for May* 15, 1850.

ON SOME PHÆNOMENA OF DEFECTIVE VISION.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN, Brompton, April 6, 1850.

One of the earliest evidences of old age creeping on is experienced in defective vision; and some months ago my attention was directed to a peculiarity in the phænomena attending it which I do not recollect to have seen noticed in books. It may possibly be peculiar to my own eyes: but I think not; for though there is a

difference between my left and right, from long habit of using microscopes, still the general result is similar in each, and I think such as must be detected by other persons.

I made a small square bounded by narrow lines on a sheet of paper, and closing my left eye, I observed that the image was double, the false one being raised, and to the left of the true the interval filled up with a pale reddish-brown spectrum, most intense near the true image; the breadth of the interval about 0·03 inch. With my right eye closed, the image was, as I expected, to the right and above (in both instances towards the brow and nose), and the interval similarly tinged, but fainter, and 0·025 inch wide.

I then took a slip of card, and made with a pen-knife a fine slit in it, and looked through it held horizontally at the square; the upper false image was no longer visible, the upper and lower sides of the square clearly defined, but the lateral sides were unchanged; on holding the slit vertically, the lateral false images disappeared and the upper and lower returned. Similar appearances occurred with the other eye.

I now took a card and made two fine pin-holes exactly in the positions of the centres of my pupils, and I found that I saw the true image as correctly as I had ever done in my life; in fact, it supplied the place of a pair of spectacles; the square, of course, being seen under a greater angle, appearing magnified. By making the pin-hole larger or smaller, the focal distance, if I may use the expression, increased or diminished proportionably. I beg further to remark, that in the sunshine I can read small print at the natural focal distance; it is only in fainter light that the double image and confusion of letters occur. Now a flattening of the cornea won't explain all this: it seems to have a more intimate connexion with some want of contractility engendered in old age in the iris. I am curious to obtain an explanation, and send this notice to you for the purpose of eliciting one.

Truly yours,

R. T. Cranmore.

P.S. I have since observed that wire-gauze, the wire being very fine, and the meshes about $\frac{1}{50}$th of an inch in diameter, when worn close to the eye, enables me to read small print with tolerable facility at a distance of five or six inches; when the meshes are closer, I can see the most minute objects with remarkable distinctness.

Brompton, May 4, 1850. R. T. C.

ON THE EXISTENCE OF IODINE IN FRESHWATER PLANTS. BY M. AD. CHATIN.

In verifying the fact stated by Muller (Lindley's Vegetable Kingdom), of the existence of iodine in a cress of unknown origin, the author has ascertained—

That iodine exists in freshwater cresses, that it is not peculiar to this species, nor general with respect to plants of the family of the Cruciferæ;

That iodine does not exist, or at any rate cannot be discovered in terrestrial plants, whereas it exists in all aquatic plants;

That of the latter, those which occur in running water are richer in iodine than those of stagnant water;

That in sheets of water, which are sufficiently large to be strongly agitated by winds, the plants approach those of running water, in the quantity of iodine which they contain;

That the proportion of iodine contained in plants is in general independent of their nature, and subordinate only to their habitat, as indicated by the Confervæ, the Potamogetons, the Nymphæa, the Ranunculi, and the Cresses, all of which are equally rich in iodine in running waters, and equally poor in marshes;

That the iodine exists uncombined with the tissue, but in the state of alkaline iodide in the juice of the plant.—*Comptes Rendus*, Mars 25, 1850.

METEOROLOGICAL OBSERVATIONS FOR APRIL 1850.

Chiswick.—April 1. Overcast: rain. 2. Cloudy: clear. 3. Fine: showery at night. 4. Boisterous, with rain. 5. Cloudy and rather boisterous: fine: clear. 6. Overcast. 7. Cloudy and fine. 8. Foggy: overcast: slight showers. 9. Cloudy. 10. Very fine. 11. Rain: showery: overcast. 12. Fine: hot sun: thunder commenced ¼ past 12 noon: overcast at night. 13. Hazy: rain. 14. Hazy: rain at night. 15. Rain: very heavy rain in forenoon: cloudy. 16. Showery. 17. Low white clouds: fine throughout. 18. Clear: very fine. 19. Rain. 20. Showery. 21. Slight rain: cloudy. 22. Fine: clear. 23. Overcast: cloudy. 24. Clear: fine. 25. Fine: overcast and cold: clear at night. 26. Cloudy and cold. 27. Fine, but cold. 28, 29. Cloudy: fine, but cold, with excessively dry air. 30. Fine: cloudy.

Mean temperature of the month	48°·41
Mean temperature of April 1849	44 ·29
Mean temperature of April for the last twenty-four years ...	47 ·53
Average amount of rain in April	1·46 inch.

Boston.—April 1. Cloudy. 2. Cloudy: rain P.M. 3. Fine. 4. Cloudy: rain A.M. 5. Cloudy. 6. Cloudy: rain P.M. 7. Cloudy. 8. Cloudy: rain A.M. and P.M. 9. Cloudy: rain P.M. 10. Fine. 11, 12. Cloudy. 13. Cloudy: rain P.M. 14. Cloudy. 15. Cloudy: rain early A.M. 16. Cloudy: rain A.M. and P.M. 17. Cloudy: rain. 18. Fine. 19. Cloudy: rain A.M. 20, 21. Cloudy: rain P.M. 22. Cloudy. 23. Cloudy: rain. 24. Cloudy. 25. Fine. 26—28. Cloudy. 29. Cloudy: rain P.M. 30. Cloudy.

Applegarth Manse, Dumfries-shire.—April 1, 2. Rain, moderate. 3. Rain: mild: heavy P.M. 4. Rain heavy during night: drizzling throughout the day. 5. Rain in the night: dry and windy. 6. Heavy showers all day. 7. Rain: cleared P.M. 8. Rain. 9. Slight shower. 10. Rain in the night. 11. Frost: clear sunshine. 12. Fine day. 13. Fine A.M.: cold and windy P.M. 14. Strong cold east wind. 15. Rain and high wind. 16. Fair and fine: slight drizzle P.M. 17. Fine: shower heavy P.M. 18. Mild: growing shower. 19. Rain early A.M.: fair day. 20. Rain early: showery day. 21. Fair and fine. 22. Cold and clear. 23. Cold and clear: slight frost. 24. Cold and clear: frost A.M. 25. Rain and wind; boisterous. 26. Fair: cold. 27. Fair: clear and cold. 28. Fair A.M.: slight shower P.M. 29. Frost early. 30. Cold: a few drops of rain.

Mean temperature of the month	46°·3
Mean temperature of April 1849	42 ·3
Mean temperature of April for the last twenty-eight years .	44 ·3
Rain in April 1849	2·52 inches.
Rain in April for twenty-three years	1·76 inch.

Sandwick Manse, Orkney.—April 1. Fog: damp. 2. Cloudy: rain. 3. Damp. 4. Rain. 5. Drizzle. 6. Cloudy: hazy A.M. 7. Cloudy: drops A.M. 8. Rain: cloudy. 9. Drizzle: showers. 10. Hazy: clear. 11. Drops: damp. 12. Bright: fine. 13. Cloudy: clear. 14. Clear: cloudy. 15. Drizzle: cloudy. 16. Rain: cloudy. 17. Cloudy. 18. Showers: bright: cloudy. 19. Cloudy: showers: cloudy. 20. Bright: cloudy. 21. Cloudy: drops. 22. Bright: clear. 23, 24. Cloudy. 25. Bright: clear. 26. Cloudy; drops. 27. Clear; fine. 28. Cloudy: fine. 29, 30. Drops; cloudy.

Meteorological Observations made by Mr. Thompson *at the Garden of the Horticultural Society at* CHISWICK, *near London; by* Mr. Veall, *at* BOSTON; *by the* Rev. W. Dunbar, *at Applegarth Manse,* DUMFRIES-SHIRE; *and by the* Rev. C. Clouston, *at Sandwick Manse,* ORKNEY.

Days of Month.	Barometer.							Thermometer.							Wind.				Rain.			
	Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		Orkney, Sandwick.		Chiswick. 1 p.m.	Boston.	Dumfries-shire.	Orkney, Sandwick.	Chiswick.	Boston.	Dumfries-shire.	Orkney, Sandwick.
1850. April.	Max.	Min.		9 a.m.	9 p.m.	9¾ a.m.	8¼ p.m.	Max.	Min.		Max.	Min.	9¾ a.m.	8¼ p.m.								
1.	29·638	29·303	29·33	29·38	29·20	29·52	29·45	61	44	47	53	43½	43	42½	se.	se.	ese.	se.	·08	·05	......	·16
2.	29·210	29·199	28·84	29·00	28·97	29·32	29·23	62	42	54	57	43½	42	40	se.	s.	s.–se.	ese.	......	......	......	......
3.	29·413	29·193	28·90	29·05	28·80	29·15	29·13	57	45	51	54	44	41½	43	sw.	sw.	s.	se.	·30	·16	......	·50
☾ 4.	29·324	29·974	28·54	28·71	28·86	28·75	28·84	57	43	55	56	46	47	44	sw.	s.	se.	se.	·03	......	......	·19
5.	29·851	29·627	29·08	29·28	29·60	29·06	29·47	59	30	49	54	46½	47	44	w.	w.	w.	wnw.	......	·01	......	·23
6.	29·719	29·687	29·27	29·48	29·40	29·47	29·46	58	45	46	50	34½	45½	42½	sw.	sw.	se-sw.	se.	·02	......	......	·05
7.	29·701	29·599	29·16	29·30	29·40	29·29	29·30	66	42	55	54	40	46	47	sw.	sw.	s-ssw.	s.	......	·04	......	......
8.	29·415	29·346	29·00	29·32	29·16	29·34	29·35	60	42	55	58	41½	48	45	s.	wsw.	se.	se.	·02	·05	1·10	·13
9.	29·415	29·315	28·87	29·06	29·02	29·22	29·23	60	38	53	53	46	45½	45	sw.	s.	ese.	se.	·01	·11	......	·14
10.	29·442	29·362	29·02	29·12	29·27	29·22	29·33	60	35	47·5	50	43½	46	44	se.	wsw.	s.	se.	·06	......	......	·07
11.	29·369	29·449	29·04	29·34	29·41	29·41	29·56	60	35	52	57	33	46	45	s.	e.	se.	se.	·05	......	......	·03
● 12.	29·827	29·620	29·25	29·62	29·72	29·72	29·84	63	37	49	57	41	49	43½	n.	n.	se.ssw	se.	·04	......	......	......
13.	29·885	29·729	29·50	29·75	29·68	29·84	29·89	54	43	45·5	55	37	45	40	sw.	nnw.	sse.	se.	·30	......	......	......
14.	29·885	29·810	29·47	29·78	29·66	29·89	29·88	54	44	46	54	38	47	42	n.	e.	e.	se.	·05	·31	......	......
15.	29·447	29·284	29·13	29·35	29·06	29·67	29·43	59	43	50	46	40	43	42	sw.	ese.	e.	ese.	·26	·03	......	·05
16.	29·173	29·096	28·74	28·98	28·98	29·10	29·07	53	42	52·5	55	43½	45	43½	s.	s.	e.	se.	·16	·41	0·55	·20
17.	29·726	29·479	29·00	29·19	29·43	29·13	29·27	61	33	53	53½	38	48	45½	sw.	w.	w.	sw.	......	·06	......	......
18.	30·058	29·907	29·44	29·70	29·75	29·58	29·72	62	35	48·5	55	42½	47½	45	w.	w.	sw.	se.	......	·03	......	·02
☽ 19.	30·110	29·834	29·48	29·59	29·59	29·44	29·44	62	44	55	58	45	48	44	sw.	s.	sw-se.	s.	·26	......	......	......
20.	29·621	29·468	29·07	29·25	29·29	29·45	29·52	59	35	53	50	42	50	46	sw.	w.	wsw.	nne.	·08	·04	......	·17
21.	29·785	29·570	29·20	29·53	29·74	29·69	29·86	58	35	49	56	40	44	42	n.	w.	nw.	n.	·07	·03	......	......
22.	29·958	29·828	29·48	29·89	29·90	30·03	30·08	59	33	47	55½	36	42	40	nw.	nw.	n.–e.	n.	......	·07	......	·12
23.	29·977	29·956	29·57	29·92	29·92	30·09	30·10	55	29	45·5	54½	35	44	40½	nw.	nnw.	ne.	nne.	......	......	1·25	......
24.	30·131	30·076	29·67	30·00	30·00	30·10	30·10	58	32	43	51	31	43	41	nw.	nw.	nw.ne	n.	......	·04	......	·05
25.	30·066	29·972	29·68	29·85	29·97	30·05	30·22	56	39	47	47	40½	48	40	se.	sse.	e.	ese.	......	......	......	·04
○ 26.	30·146	30·065	29·77	30·18	30·19	30·31	30·36	61	41	44	53	40	46	43	ne.	ne.	ene.	se.	......	......	......	......
27.	30·179	30·146	29·80	30·24	30·24	30·40	30·45	55	34	48	55	41	44½	42½	e.	ene.	ese.	......	......	......	......	......
28.	30·296	30·225	29·90	30·29	30·32	30·45	30·50	55	33	47	50½	38	47	42	e.	ene.	e.	......	......	......	......	......
29.	30·336	30·282	30·00	30·39	30·32	30·51	30·48	57	35	46	47	31½	44	42	e.	ne.	ne.	nnw.	......	......	......	·02
30.	30·246	30·146	29·83	30·28	30·16	30·35	30·30	57	39	47	48	38½	47	41	ne.	ne.	ne.	n.	......	·03	0·03	·03
Mean.	29·778	29·684	29·30	29·560	29·567	29·651	29·695	58·60	38·23	49·3	53·2	40·0	45·63	42·91					1·79	1·47	2·93	2·20

THE

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SUPPLEMENT TO VOL. XXXVI. THIRD SERIES.

LXII. *On the Geometrical Interpretation of Quaternions. By* W. F. DONKIN, *M.A., &c., Savilian Professor of Astronomy in the University of Oxford**.

IN a recent paper in this Journal, I gave two examples of the application of quaternions to geometrical problems, in which I made use of a system of interpretation different from that usually employed by Sir W. Hamilton. This system it is my present object to explain. I am not at present prepared to offer an opinion as to its practical advantages or disadvantages. It is here proposed as possessing a certain *theoretic* interest, arising from the fact that it represents the method of quaternions as a natural (or rather *the* natural) extension to tridimensional space of the usual geometrical interpretations of symbolical algebra. The general principles employed in the investigation have no pretensions to novelty. The view here taken of symbolical algebra, and of its interpretation in plane geometry, is substantially the same as that advocated by the late Mr. Gregory (in the fourteenth volume of the Edinburgh Philosophical Transactions, and in several papers in the Cambridge Mathematical Journal); and the extension to geometry of three dimensions rests upon principles which will be found, I believe, to agree essentially, as far as they go, with those laid down by Sir W. Hamilton in his papers on symbolical geometry in the last-named periodical. In what follows, then, no further reference will in general be made to previous writers. Moreover, to avoid the use of frequent qualifying phrases, propositions will be stated dogmatically which only claim to be accepted as belonging to *a consistent system,* not as belonging to the *only* consistent or useful system.

General symbolical algebra is a calculus of operations. All the symbols employed, including those arithmetical symbols which occur, represent or indicate operations. The *subject*

* Communicated by the Author.

of operation, or (to adopt Sir W. Hamilton's convenient term) the *operand*, is in general not represented by any symbol at all, but is to be understood. Thus the symbol ab represents the successive performance of the operations b, a upon the operand. Neither operations nor operand are *defined*, further than definition is involved in the assumed laws of the combination of symbols. If we assume $ab=ba$, we limit to a certain extent the choice of meanings for the *operations* a and b; and if we assume that the arithmetical equation $m+n=s$ shall be true in symbolical algebra (where m and n are absolute numbers, and s their sum), we limit the choice of the *operand*: for let it be a; then the above equation signifies $m\mathrm{a}+n\mathrm{a}=s\mathrm{a}$, and the meaning of a must be such that this may be capable of an intelligible and consistent interpretation. It is not necessary for our present purpose to attempt a complete discussion of the laws of the usual algebraical symbols. The following remarks, however, may serve to elucidate the processes to be explained hereafter.

The symbols $+$ and $-$ are used in algebra in two essentially different ways. First, they are used, as in arithmetic, to indicate addition and subtraction. Thus $a+b$ denotes a certain *resultant* formed by separately performing the operations a and b on the *operand*, and *adding* the result of the latter operation to that of the former. The meaning of *addition* cannot be defined till the nature of the operand is determined. The same may be said of $a-b$, *mutatis mutandis*. The equations

$$a+(b+c)=a+b+c, \text{ \&c.}$$

suggested by arithmetic as convenient assumptions, suggest in their turn *further* assumptions; namely, that if $+$ and $-$ should ever be used as *true symbols of operation*, so that $+\mathrm{a}$ should denote the result of performing the *operation* $+$ upon the *operand* a, and $+-a$ should denote the performance of the successive operations a, $-$, $+$ upon an operand (not expressed), *then* the laws of their combinations should be

$$++=--=+, \quad +-=-+=-.$$

Such combinations do not really occur at all in the *first* use of these symbols. The expression $a-(b-c)$ only *suggests*, but does not really *contain* the combination $--$.

We now come to the *second* way in which $+$ and $-$ are used, namely, as true symbols of operation, defined by the laws of combination just stated.

Let us for the present distinguish this use of the symbols by inclosing them in brackets. Then the following equations

are well known, viz.

$$(+)^n=(+), \quad (-)^{2n}=(+), \quad (-)^{2n-1}=(-),$$

where n is an integer. But it is to be remembered, that, unless $n=1$, these equations do not express *identity*, but only *equivalence*. $(-)^2=(+)$ is an identical equation; but $(+)^2=(+)$ does not mean that $(+)^2$ is the *same operation* as $(+)$, but only that it produces the *same result* when performed on the operand. Thus $(+)^2=(+)$ is only true in the same sense in which $(+)=1$ is true. And this brings us to a point of considerable importance, namely, the twofold use of the symbol 1 in algebra, as representing an *operation* on the one hand, or *a concrete quantity* or *operand* on the other. Considered in the former light, 1 represents the *operation* of *taking the operand as it is*; considered in the latter, it represents a *concrete unit* taken as a *subject* of operation. In the former case $(+)1$ and $(-)1$ may be used as synonymous with $(+)$ and $(-)$, and $\sqrt{(+)1}$, $\sqrt{(-)1}$ as synonymous with $(+)^{\frac{1}{2}}$, $(-)^{\frac{1}{2}}$. In the *latter* case

$$\big((+)1\big)^2, \quad \big((-)1\big)^2, \quad \sqrt{(+)1}, \quad \sqrt{(-)1}$$

are all equally unmeaning, just as

$$\big((+)£1\big)^2, \quad \sqrt{(-)£1}, \text{ \&c.}$$

are equally unmeaning. It appears to me that a great deal of confusion has arisen from neglecting this distinction. At all events I intend to preserve it strictly in the present paper, in which *all* the symbols will represent *operations*, and never *concrete quantities*, unless that be expressly stated. Thus, if x be one of the coordinates of a point in space, x represents, strictly speaking, in any equation in which it may occur, not a line, nor a number, but the *operation* of multiplying the operand (which is understood throughout) by the same number by which the linear unit must be multiplied in order to produce a line equal to the distance of the point in question from the plane of yz. If this be well understood, there is no objection to speaking of x, for convenience, as if it really represented such a line.

To return, however, to the symbols $+$ and $-$, the equations

$$a+(+)b=a-(-)b=a+b, \quad a-(+)b=a+(-)b=a-b,$$

which rest on grounds that need not be here examined, justify us in omitting the use of brackets to save trouble, though it would perhaps be theoretically desirable to have some equivalent mark of distinction.

The principles of the usual interpretation of symbolical

algebra in plane geometry are now generally understood and recognized. There are some important remarks, however, to be made with reference to what follows. The *operand*, in this system of interpretation, is always a *directed line*, that is, a straight line in a determinate direction, and with a determinate *beginning* and *end*. It is convenient for our present purpose to assume that *all* the lines considered shall have their beginnings at the same point or origin. The operand, then, is a radius vector drawn from a determinate origin in a determinate plane. The symbol + denotes the operation of turning this line round the origin through a whole revolution, in a determinate sense, which we will assume to be contrary to that of the motion of the hands of a watch. It is especially important to observe, that + does *not* represent the operation of *placing the line in a determinate direction* (such as that of a given fixed axis), but the operation of *turning it round from the direction it had at first* (which may be any whatever) *till it comes into the same direction again.* Then $(+)^{\frac{1}{2}}$, or $-$, represents a semi-revolution; $(-)^{\frac{1}{2}}$, or, as it is commonly written, $\sqrt{-1}$, a quarter of a revolution; and so on. And if a be a numerical symbol, then $(+)^{\alpha}a$ represents the compound operation of altering the length of the operand line in the ratio of a to 1, and also turning it round through an angle equal to $2\alpha\pi$.

The *addition* and *subtraction* of directed lines is to be performed, as is well known, according to the rules for the composition of forces. Thus, if a, b be two radii vectores, a+b represents another radius vector, namely the diagonal (drawn from the origin) of the parallelogram constructed on them; and a−b represents a third radius vector, namely the diagonal (from the origin) of the parallelogram of which two sides are a, and a line equal to b, but in the opposite direction. The symbol $\cos\theta+\sqrt{-1}\,.\sin\theta$ represents a compound operation, namely (1) multiplying the operand by $\cos\theta$ without altering its direction; (2) multiplying it by $\sin\theta$ and turning it through a right angle; (3) adding together the two lines so obtained, or taking the diagonal of the parallelogram described upon them; which diagonal is obviously equal in length to the original operand, but inclined to it at an angle θ, so that $\cos\theta+\sqrt{-1}\,.\sin\theta$ represents the operation of turning through an angle θ, and is equivalent to $(+)^{\alpha}$, if $\theta=2\alpha\pi$. Nothing depends, as was before observed, on the direction of the operand line; but in interpreting such expressions as $p+q$, where p, q are any symbols of operation, of course it is assumed that both refer to the *same operand.* In plane geometry, the

direction of the operand line is in general completely arbitrary in its own plane; but this is not generally true when it is a line in space *not* confined to a determinate plane. I now proceed to consider the application of algebraical symbols to this case.

If we assume the symbol $+_r$ to represent the operation of turning a line through a complete revolution in a plane perpendicular to a determinate axis r, drawn from the origin, then $(+_r)^\alpha$ will represent the operation of turning through an angle $2\alpha\pi$; and we may use $-_r$ as a symbol equivalent to $(+_r)^{\frac{1}{2}}$, denoting a semi-revolution. So far as rotations in this one plane are concerned, all that has been said of $(+)$ and $(-)$ in plane geometry will apply to $+_r$ and $-_r$. I will add, however, one remark, which may be useful to readers (if this paper should meet with such) to whom the subject is new. If we admit (which, however, is not necessary) the idea of *negative angles*, then, θ representing an *angle* described by a determinate rotation, $-\theta$ represents an equal angle described by a contrary rotation. But if q represent the *operation* of describing the angle θ, or the rotation by which it is described, then the operation of describing $-\theta$ is represented, not by $-q$, but by $\frac{1}{q}$ or q^{-1}. The equation $\theta+(-\theta)=0$ means the same thing as $q^{-1}q=1$. The former expresses that the angular distance between the first and last positions of the describing line is 0; the latter, that if we turn a line through any angle and then back again, the result is equivalent to simply *taking it as it is*, which, considered as an *operation*, is represented by the symbol 1. To justify the assertion that the consideration of negative angles is not necessary, it will be sufficient to observe that an angle which is negative when considered as described about one axis, is positive if it be considered as described about an opposite axis. But a complete discussion of this point would involve an examination of the theory of indices, and would lead us too far from our immediate subject, to which I return.

For convenience let q be put for $(+_r)^\alpha$, so that q is a symbol of rotation, and represents the operation of turning the operand line through an angle $\theta(=2\alpha\pi)$ in a plane perpendicular to a given line r (whose length is immaterial) drawn from the origin; the direction of rotation, moreover, being such that the line r shall be its *positive* or *north* axis. The initial position of the operand must be *in the plane of the rotation*, otherwise the operation could not be performed upon it: but so far as this one operation is concerned, it may be in *any*

part of the plane; and the symbol q represents, indifferently, rotation through an angle θ in *any part of the plane.*

Now let $q' = (+_{r'})^{\beta}$ represent in like manner the operation of turning through an angle $\theta' = 2\beta\pi$ in another plane whose axis is r'. The two operations q, q' cannot in general be successively performed upon the same operand line. In order that this may be possible, it is necessary to place it at first in the plane of the first rotation q, and in such a direction that this first rotation shall bring it into the line of intersection of the two planes, whereby the second rotation becomes possible.

Let q'' represent the single rotation which would have brought the line from its initial position into that final position in which it is placed by the successive rotations q, q'; we have then the symbolic equation (or rather equivalence) $q'q = q''$. The successive rotations q, q' are (not identical with, but) equivalent in effect to the single rotation q''. If now a third rotation q''' is to be performed, the rotation q'' must (if necessary) be transported in its own plane so as to make the compound operation $q'''q''$ possible, just as the first rotation q was transported so as to make $q'q$ possible. These conventions enable us to assign a determinate rotation in a determinate plane as the result (or product) of any number of given successive rotations in given planes. But it is obvious that no useful method of calculation could be based upon such assumptions, unless we could prove (as we can prove) that the *associative principle* holds good with respect to the product of rotations; that is, that $(q''q')q = q''(q'q)$.

Let us now adopt a method of representing rotations (in words or by actual diagrams) similar to that employed in the previous paper. Conceive a sphere with arbitrary radius to be described about the origin; and, AB being any arc of a great circle on this sphere, let "*the rotation* AB" signify that rotation of a radius vector which would cause its intersection with the sphere to describe the arc AB (*from* A *to* B), *or any equal and similarly described arc in the same great circle.* [It must be carefully borne in mind, that we are now concerned with the rotations of *lines*, and not, as in the former paper, of *solids*].

Let ABC be any spherical triangle, and let the rotations AB, BC, AC be represented as above by q, q', q''; we have then $q'q = q''$. It is easy to see that qq' does not represent the same thing as $q'q$, and also to see what it *does* represent. For producing AB to A′ and CB to C′, making BA′ = AB, BC′ = CB, and joining C′A′, we see that qq' represents the successive rotations C′B, BA′, the effect of which is the same as that of C′A′. If, then, we denote th s last rotation by $q_{\prime\prime}$;

we have $qq' = q_{\prime\prime}$; and since C′A′ is equal to AC, we see also that qq', $q'q$ represent *equal* rotations, but generally in *different planes*; these planes, however, being equally inclined to the planes of q, q'.

Consider now in particular the case in which AB, BC are both quadrants. Then it is obvious that C′A′ is in the same great circle with AC, so that if q'' represent the rotation AC, $\frac{1}{q''}$ will represent the rotation C′A′, and we have therefore, *in this case*, $qq' = (q'q)^{-1}$. Now let $q_{\prime}$ represent (not AC but) CA, so that q, q', $q_{\prime}$ represent the rotations by which the sides of the triangle would be described in a cyclic order; then we shall have, for *any* triangle,

$$q'q = \frac{1}{q_{\prime}}, \quad qq_{\prime} = \frac{1}{q'}, \quad q_{\prime}q' = \frac{1}{q};$$

and if we further suppose *all the sides of the triangle to be quadrants*, we shall also have, as we have just seen,

$$qq' = q_{\prime}, \quad q_{\prime}q = q', \quad q'q_{\prime} = q.$$

Before we proceed further, there is one important remark to be made with respect to the symbol $(+_{r})^{\frac{1}{2}}$, or $-_{r}$; namely, that the effect of the rotation denoted by it, is, regard being had to the conventions above established, *independent of the direction of the axis* r; since a semi-revolution, in *any* plane in which it is possible, merely *reverses the direction of the operand line.* We may therefore in all cases substitute the general symbol $-$ for $-_{r}$, and write $(+_{r})^{\frac{1}{2}} = -$. To illustrate this further, let q represent any rotation AB, and let us examine the meaning of the product $-_{r'}q$. The arc AB must be so placed in its own plane that its end B shall be at the intersection of that plane with the plane of the rotation $-_{r'}$; then the operand line must first describe AB, and then half a circumference in the latter plane, after which it will obviously be again in the same great circle with AB, cutting the sphere in the point opposite to B. Now the same effect would have been produced by a single rotation in the plane of the rotation q, through an angle $\theta + \pi$ described positively, or $\pi - \theta$ described negatively, θ being the angle of the rotation q. If, then, $q = (+_{r})^{\alpha}$, we have

$$(-_{r'})q = (+_{r})^{\alpha \pm \frac{1}{2}},$$

in which we may henceforth drop the subscript r', and write simply $-q$ on the first side.

It is still more obvious that similar remarks apply to the case of the symbol $+_{r}$; or that we may always drop the sub-

script, and use $+$ simply, to denote a *whole* revolution in *any* plane. But there are no other values of α besides these two of $\alpha=1$, $\alpha=\frac{1}{2}$, and their multiples, for which the effect of the rotation $(+_r)^\alpha$ is independent of the direction of r. It is also to be noticed, that these are the only cases in which the symbol $(+_r)^\alpha$ is *commutative* with respect to all other symbols of rotation. The reader will easily see, that, on the principles we have admitted, $(+_r)^{\frac{1}{2}}q$ is equivalent to $q(+_r)^{\frac{1}{2}}$. Either will be henceforth denoted by $-q$.

Now let there be three fixed rectangular axes meeting the sphere in X, Y, Z, and so arranged that X is the north pole of the rotation YZ. Let us for convenience assume three fixed symbols, i, j, k, to represent the three rotations YZ, ZX, XY; that is, quadrantal rotations respectively in (*any parts of*) the three planes of yz, zx, xy. According to our previous notation, we might write

$$i=(+_x)^{\frac{1}{4}},\quad j=(+_y)^{\frac{1}{4}},\quad k=(+_z)^{\frac{1}{4}};$$

but we shall not have much further occasion for it.

The conclusions obtained above respecting quadrantal triangles give the equations

$$jk=i,\quad ki=j,\quad ij=k,\quad kj=\frac{1}{i},\quad ik=\frac{1}{j},\quad ji=\frac{1}{k};$$

of which the three last can now be written

$$kj=-i,\quad ik=-j,\quad ji=-k.$$

For we have

$$i^{-1}=(+_x)^{-\frac{1}{4}}=(+_x)(+_x)^{-\frac{1}{4}}=(+_x)^{\frac{3}{4}}=(+_x)^{\frac{1}{2}}(+_x)^{\frac{1}{4}}=-_x i;$$

and we have seen that $-_x$ may be replaced by $-$, so that we get $i^{-1}=-i$, and, in like manner,

$$j^{-1}=-j,\quad k^{-1}=-k.$$

Also we have

$$i^2=(+_x)^{\frac{1}{2}}=-_x,$$

and similarly,

$$j^2=-_y,\quad k^2=-_z;$$

which may now be written

$$i^2=j^2=k^2=-\ ;$$

and, to conform to common notation, we may put -1 instead of $-$. In all these cases it is to be recollected that the symbol $=$ denotes, not identity of operations, but equivalence of results.

The associative principle, so far as the products of the quadrantal rotations represented by i, j, k are concerned, is now established, since

$$(ij)k=k^2=-1, \quad i(jk)=i^2=-1, \text{ \&c.}$$

Hitherto we have been considering the use of the symbol $+_r$ as representing *rotation.* We must now proceed to examine the principles on which the symbols $+$ and $-$ are to be employed in the present system, as indicating the addition and subtraction of directed lines.

Let q, q' denote any two rotations, and a, a' two numbers. What is the interpretation of $aq+a'q'$? In the first place, aq represents the compound operation of turning the operand line through the rotation q, and altering its length in the ratio of a to 1; and $a'q'$ has a similar meaning. Next, in order that aq, $a'q'$ may both refer to the *same* operand line, that line must at first coincide with the intersection of the planes of the two rotations q, q'. Let a be the operand so placed; then $aq\mathrm{a}$, $a'q'\mathrm{a}$ represent two lines whose lengths are $a\mathrm{a}$, $a'\mathrm{a}$, and whose directions are determined by the rotations q, q'. Their sum $aq\mathrm{a}+a'q'\mathrm{a}$ represents the diagonal of the parallelogram constructed upon them. Let $a''\mathrm{a}$ be the length of this diagonal, and q'' the rotation which would bring the original operand into coincidence with it; then we have

$$aq\mathrm{a}+a'q'\mathrm{a}=a''q''\mathrm{a},$$

or omitting the symbol of the operand,

$$aq+a'q'=a''q''.$$

Thus the complex symbol of operation, $aq+a'q'$, represents a single determinate rotation combined with a determinate alteration of length. It is easy to illustrate this by a diagram, thus:—

Let ABC be any spherical triangle, and let q, q' represent the rotations AB, AC. Then $(aq+a'q')\mathrm{a}$ represents the diagonal of a parallelogram constructed on two radii vectores which cut the sphere in B and C, and whose lengths are $a\mathrm{a}$, $a'\mathrm{a}$. This diagonal will cut the sphere in a point D of the arc BC, such that

$$\sin \mathrm{BD} : \sin \mathrm{CD} :: a' : a.$$

Join AD; then, in the equation $aq+a'q'=a''q''$, we have obviously

$$a''^2=a^2+a'^2+2aa' \cos \mathrm{BC},$$

and q'' is the symbol of the determinate rotation AD.

The expression $a+a'q'$, which is merely a particular case of the above, is easily interpreted in the same way. The ope-

rand a must first be in the plane of the rotation q'. The operation a merely alters its length into aa; the operation $a'q'$ alters its length into a'a, and also alters its direction. Then $(a+a'q')$a is the diagonal of the parallelogram constructed on the two lines represented by aa, $a'q'$a; and if a''a be the length of this diagonal, and q'' the rotation which would bring the operand into coincidence with it, we have, as before,

$$a+a'q'=a''q''.$$

The expression

$$aq+a'q'+a''q'',$$

in which q, q', q'' represent *any* three rotations, and a, a', a'' any three numbers, cannot generally be interpreted without the help of conventions similar to those adopted in interpreting products; because unless the planes of the three rotations happen to intersect in the same line, it is impossible so to place the operand that each operation can be performed on it. But we have seen that $aq+a'q'$ is equivalent to the symbol of a single determinate rotation in a determinate plane, combined with a determinate alteration of length. Suppose, therefore,

$$aq+a'q'=a_{\prime}q_{\prime}.$$

Then we may transport the rotation $q_{\prime}$ in its own plane, until its origin coincides with the line of intersection of that plane with the plane of the rotation q'', so that $a_{\prime}q_{\prime}+a''q''$ may be interpretable in the same way as $aq+a'q'$. Thus we shall finally arrive at a single determinate rotation, with a determinate alteration of length, as the interpretation of

$$(aq+a'q')+a''q''.$$

The *usefulness* of the system will depend upon our being able to show (as we shall easily do) that the associative principle holds good in addition, or that

$$(aq+a'q')+a''q''=aq+(a'q'+a''q'')=(aq+a''q'')+a'q',$$

which will justify us in omitting the brackets.

The case of *subtraction* is evidently included in that of addition, since the sign $-$, preceding any symbol of rotation, may be changed into $+$, provided we increase the angle of rotation by two right angles.

We are now in a position to prove the following fundamental proposition:—

Let i, j, k represent, as above explained, quadrantal rotations whose positive poles are respectively X, Y, Z, and let l, m, n be the direction cosines of any line r from the origin. Then $il+jm+kn$ represents *a quadrantal rotation* whose po-

sitive axis is r; or, according to our previous notation,

$$il+jm+kn=(+_r)^{\frac{1}{2}}.$$

If the condition

$$l^2+m^2+n^2=1$$

did not subsist, then, putting

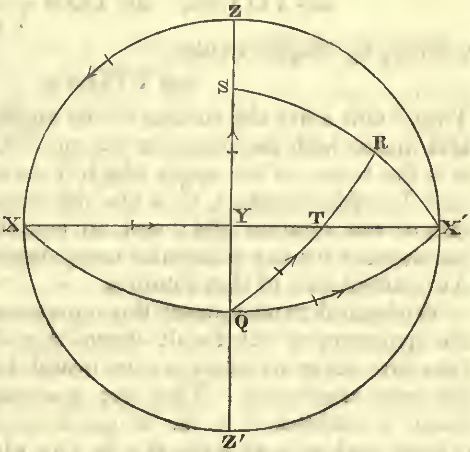
$$\sqrt{l^2+m^2+n^2}=\mu,$$

we should have

$$il+jm+kn=(+_r)^{\frac{1}{2}}\mu,$$

representing the compound operation of turning the operand through a right angle in the plane perpendicular to r, and altering its length in the ratio of μ to 1, the direction cosines of r being *proportional* to l, m, n.

The proof of this is very simple, but can hardly be given without a diagram. In the figure (of which all the lines represent arcs of great circles), let X, Y, Z be the positive poles of the rotations i, j, k, or the points in which the positive axes of x, y, z cut the sphere; and let X′, Z′ be diametrically opposite to X, Z.



Now to interpret $mj+nk$, take XZ′ and XY to represent the rotations j, k; then taking Q in YZ′, so that

$$\sin YQ : \sin QZ' :: m : n,$$

and joining XQ, we have evidently, by the principles established above,

$$mj+nk=\sqrt{m^2+n^2}.q,$$

where q represents the quadrantal rotation XQ. Next, to interpret

$$\sqrt{m^2+n^2}.q+li,$$

transport the rotation q in its own plane to QX′, so that its origin may be in the plane of the rotation i. Take QS a quadrant, and join SX′. Take R in SX′ so that

$$\sin SR : \sin RX' :: \sqrt{m^2+n^2} : l,$$

and join QR. Then, by the same principles, we have

$$\sqrt{m^2+n^2}.\,q+li=\sqrt{l^2+m^2+n^2}.\,q',$$

where q' represents the quadrantal rotation QR. If we suppose

$$l^2+m^2+n^2=1,$$

then what we have just proved is that

$$il+(jm+kn)$$

represents the quadrantal rotation QR, without alteration of the length of the operand.

Let QR cut YX$'$ in T. Then in the right-angled triangle QYT the preceding conditions give easily

$$\cos \mathrm{YQT}=l, \quad \cos \mathrm{YQ}=\frac{n}{\sqrt{m^2+n^2}},$$

whence, by Napier's rules,

$$\cos \mathrm{YTQ}=n.$$

Thus l and n are the cosines of the angles which the plane of QR makes with the planes of yz, xy; whence it follows that m is the cosine of the angle which it makes with the plane of xz. In other words, l, m, n are the direction cosines of the axis of the rotation QR; and an examination of the figure constructed for any particular case, shows that they belong to the *positive* axis of that rotation.

We have thus interpreted the expression $il+(jm+kn)$; and the symmetry of the result shows that the interpretation of $(il+jm)+kn$ or of $(il+kn)+jm$ would have conducted us to the same conclusion. Thus the associative principle of addition is established so far as quadrantal rotations are concerned, and we may write $il+jm+kn$ without brackets.

Consider now the expression

$$\cos\theta+\sin\theta(il+jm+kn).$$

The first term represents the operation of altering the length of the operand in the ratio of $\cos\theta$ to 1; and the second, the compound operation of altering its length in the ratio of $\sin\theta$ to 1, and turning it through a right angle in a determinate plane. Applying, therefore, the principles explained above with reference to the interpretation of such expressions as $a+a'q'$, we see that the complex symbol now under consideration represents the operation of turning the operand, without altering its length, through an angle θ, in a plane perpendicular to the axis whose direction cosines are l, m, n. Let r denote such an axis; then we may write

$$\cos\theta+\sin\theta(il+jm+kn)=(+_r)^{\alpha},$$

where
$$\alpha = \frac{\theta}{2\pi}.$$

The quaternion
$$w + ix + jy + kz$$
is always reducible to the form
$$\mu\left(\cos\theta + \sin\theta(il + jm + kn)\right),$$
where
$$l^2 + m^2 + n^2 = 1,$$
by the assumptions
$$\mu = \sqrt{w^2 + x^2 + y^2 + z^2},\ r = \sqrt{x^2 + y^2 + z^2},\ \mu\sin\theta = r,\ \mu\cos\theta = w,$$
$$\frac{l}{x} = \frac{m}{y} = \frac{n}{z} = \frac{1}{r};$$
and therefore it represents a rotation, combined with an alteration of length in the ratio of μ to 1.

It must be observed, however, that we have so far only interpreted the expression
$$w + (ix + jy + kz),$$
and that we are not at liberty to remove the brackets without first establishing the associative principle of addition with respect to symbols of *alteration of length*, such as w, and symbols of *alteration of length combined with quadrantal rotation*, such as ix, &c. But this is very easily done, and as easily for *any* rotations as for quadrantal.

Let ABC be any spherical triangle, and let q, q' represent the rotations AB, AC. Let a, b, c be numerical quantities. Then the equations
$$(a + bq) + cq' = a + (bq + cq') = (a + cq') + bq,$$
are easily seen to express, that if a parallelepiped be constructed with three edges coinciding in direction with the radii drawn to the points A, B, C, and proportional in length respectively to a, b, c, then the diagonal of the parallelepiped may be obtained indifferently by taking the sum of any one of these edges and the diagonal of the parallelogram constructed on the other two.

We have now established* all that is necessary to give demonstrative force to geometrical conclusions deduced from algebraical calculation with quaternions. This paper has,

* The geometrical proof of the *distributive* property, expressed by such equations as
$$i(a + jb) = ia + kb,$$
&c., is so easy and obvious, that I leave it to the reader.

however, already extended to such a length, that I will only give one simple instance of such reasoning.

Let ABC be any spherical triangle, and q, q', q'' represent the rotations AB, BC, AC. Then we have, as before explained, $q''=q'q$. But if we put $AB=\theta$, $BC=\theta'$, and call l, m, n, l', m', n' the direction cosines of the positive poles of AB, BC, then we have seen that

$$q=\cos\theta+\sin\theta(il+jm+kn),$$

and q' is similarly composed of accented letters; hence

$$\begin{aligned} q'q= &\cos\theta\cos\theta'-\sin\theta\sin\theta'(ll'+mm'+nn')\\ &+i\{l\sin\theta\cos\theta'+l'\sin\theta'\cos\theta+(m'n-mn')\sin\theta\sin\theta'\}\\ &+j\{m\sin\theta\cos\theta'+m'\sin\theta'\cos\theta+(n'l-nl')\sin\theta\sin\theta'\}\\ &+k\{n\sin\theta\cos\theta'+n'\sin\theta'\cos\theta+(lm'-l'm)\sin\theta\sin\theta'\}. \end{aligned}$$

This is the quaternion symbol of the rotation AC; its first term therefore expresses cos AC, which agrees with the fundamental theorem of spherical trigonometry; and the terms affected by i, j, k are proportional to, and determine, the direction cosines of the positive axis of AC. The product qq' in like manner represents the rotation C′A′, if A′, C′ be points taken in AB, CB produced, so that $BA'=AB$, $BC'=CB$.

The reader who is familiar with Sir W. Hamilton's researches on quaternions will observe that these and similar results, which are here *primary*, appear in his system of interpretation as *secondary* or *polar*; and the converse would easily be shown to be true also. (See particularly Irish Transactions, vol. xxi. part 2, p. 80–86.) The reason of the difference is apparent. In Sir W. Hamilton's *geometrical* system, i, j, k are not symbols of operation, but represent concrete units, namely, unit lines in fixed directions, so that $ix+jy+kz$ represents also, not an operation, but a concrete quantity, namely the radius vector of the point whose coordinates, referred to axes coinciding with the directions of i, j, k, are x, y, z. The quaternion $w+ix+jy+kz$ therefore represents what may be called a *couple*, or sum of two *heterogeneous quantities*, namely, an abstract length w, and a directed line $ix+jy+kz$. It is therefore not capable of a geometrical interpretation. This circumstance, however, as Sir W. Hamilton has abundantly shown, does not at all interfere with the use of quaternions in obtaining geometrical results.

In the system of interpretation which has been explained in this paper, every symbol is regarded as representing an *operation*; and every term of the quaternion, as well as the whole expression, has a geometrical interpretation. In fact, a qua-

ternion expresses, in this system, what Sir W. Hamilton has called a *geometrical quotient*, or the operation which must be performed on one directed line to make it coincide in length and direction with another directed line. If a, a′ be two such lines, l, m, n, l', m', n' their direction cosines, and μ the arithmetical ratio of the length of a to that of a′, then the geometrical quotient $\frac{\mathrm{a}}{\mathrm{a}'}$ is expressed, as it is easy to see, by the quaternion

$$q=\mu\{ll'+mm'+nn'+i(m'n-mn')+j(n'l-nl')+k(l'm-lm')\},$$

and $\frac{\mathrm{a}'}{\mathrm{a}}$ by another quaternion obtained from q by writing μ^{-1} instead of μ, and changing the signs of the three terms affected by i, j, k. According to this view, since i, j, k are symbols of operation and not of quantity, there is nothing in the least strange in their not being *commutative* symbols, for we are familiar with non-commutative *operations* in other parts of analysis. I am not without a hope that this, with other considerations suggested by the preceding investigations, may tend to obviate any prejudice which may be felt against the theory of quaternions, as containing something arbitrary, mysterious, opposed to common notions, and incapable of interpretation. I trust it has sufficiently appeared that the theory may be completely divested of any such characteristics. At the same time I am far from thinking that they really apply, in any sense implying an objection, to Sir W. Hamilton's mode of considering the subject. On the contrary, that mode possesses, in my estimation, besides its undoubted practical utility, the attraction of suggesting a novel and very interesting subject of inquiry; the properties, namely, of *sets* or *sums* of *heterogeneous quantities*. This, however, is a topic upon which I cannot here attempt to enter.

Oxford, May 18, 1850.

LXIII. *On the Electricities of Steam.* *By* Reuben Phillips, *Esq.*

[Continued from p. 108.]

94. A COMMON laboratory brass tripod stand with straight legs was insulated, by putting each leg into a glass tube previously closed at one end by the blowpipe; the glass tubes were securely jammed on by means of worsted, and a small piece of cork was placed in the bottom of each tube, to prevent the metal bearing on the glass; the

tubes were varnished externally with a solution of sealing-wax. The longer arm of the tin pipe was placed perpendicularly to the horizon, with its mouth upwards; the other arm of the pipe stood before the brass jet of the boiler (40.), as formerly (83.). The tripod stand was placed near the tin pipe, and the fountain (77.) placed upon the tripod. The cock of the fountain was fitted with a straight glass tube, except that near its upper end the glass tube was bent twice at a right angle in the same plane; this end of the glass tube held a brass jet, through which the water was projected downwards about the axis of the long arm of the tin pipe. The distance of the brass jet of the fountain, from the nearest part of the tin pipe, was 11 inches; and the diameter of the aperture of the brass jet of the fountain was $\frac{1}{93}$ inch.

95. The fountain was now electrically connected with the ground, and the tin pipe with the single-leaf electrometer. The cock of the fountain was opened a little, so that the water was discharged from the jet by some low pressure, perhaps equal to 6 or 10 vertical feet of water; a positive charge was communicated to the electrometer. The cock of the boiler was now opened so as to make the boiler positive; a great increase of positive electricity was immediately given to the tin pipe, the gold-leaf being kept in contact with, or rapidly striking the conducting plate of the electrometer; and this effect continued without diminution so long as the supply of steam and water was maintained. This result is identical with that previously obtained (84.).

96. The fountain was now insulated; the cock of the fountain being opened as before, the positive charge, which at first was freely given to the electrometer, soon became very feeble (it could be immediately renewed by touching the fountain): then opening the boiler as before, a quantity of positive electricity passed through the electrometer; presently this effect also ceased, and then the electrometer generally began slowly to discharge itself of negative electricity. The steam was now shut off; then, as soon as the steam began to leave the tin pipe, the gold-leaf commenced rapidly striking the conducting plate with negative electricity; in a few seconds this negative charge was exhausted.

97. The negative electricity which the instrument slowly acquired in this experiment, after the positive effect was over and while the steam was passing, was no doubt merely the negative electricity collected from the steam (81.).

98. The fountain and tin pipe were now electrically united; on opening the cock of the fountain as formerly, no electricity was produced; and when the steam was sent through the tin

pipe, the negative charge collected from the steam was the only electrical effect observed.

99. During the above experiments the boiler was uninsulated, and it was examined at each experiment to be quite certain that the issue of steam communicated positive electricity to the boiler; this was, however, a needless precaution. These results have been obtained with very different pressures in the boiler, up to 40 lbs. on the inch. The statement (48.), that at 40 lbs. on the inch the boiler is positive at a certain pressure, and negative at lower, is an error, into which I fell probably through not being aware of the influence of water (74. 114.). Although I experienced considerable difficulty at first, I can, now that the conditions are better known, make the boiler positive with the same precision, whether the pressure in the boiler is 6 or 40 lbs. on the inch.

100. I have sometimes observed the electricity of the water as it issued from the fountain to be negative; at such times the electricity apparently given by the steam in the tin pipe, was negative when the steam was turned on, and positive when it was shut off, the general arrangement of the apparatus being as before.

101. The following is, I think, the explanation of these results:—When the fountain is insulated and a current of electrified water is passing from the jet, the fountain soon takes its maximum electric charge, the issuing stream of water being oppositely electrified; and the electric difference between the water and the brass jet being so great, that the electricity generated by the friction runs back again through the water to the brass jet. If now a medium possessing greater inductive capacity than air be made to surround the stream of water, the electric difference between the issuing water and the brass jet is diminished, and electricity is again carried forward by the water; and the same quantity of opposite electricity is accumulated on the brass fountain. When air is again made to surround the jet of water, the excess of electricity which the former medium had caused to be accumulated on the fountain, passes off by the jet of water. I thus come to the conclusion that the specific inductive capacity of the steam-cloud is much greater than air. It is now necessary to separate these experiments with the fountain from those of the electricity of condensation.

102. The hole in the bung was reduced to a circular aperture, ·6 inch diameter, and the distance of the aperture from the end of the brass jet of the boiler was 1·6 inch, everything else being as before (95.). The positive electricity when the steam was turned on, and the negative when it was shut

off, were much diminished. The negative electricity collected from the steam (97.) remained unaltered.

103. The hole in the bung was again reduced to a circle ·4 inch diameter; the distance of the hole from the brass jet of the boiler was 1·2 inch. When the boiler was opened as usual, the only electrical effect I could certainly recognize was the negative electricity of the steam, which remained just as before.

104. It follows, from the foregoing, that it is the steam-cloud, and not the steam, which possesses the large capacity for induction. Indeed as much might have been inferred from the fact discovered by Dr. Faraday, that gaseous bodies have but one, or nearly one inductive capacity.

105. The glass tube was taken from the fountain, and the brass jet of the fountain was removed from the glass tube, and united to the cock of the fountain so that the stream of water might issue horizontally; the fountain being set on the tripod stand as in former experiments. One end of a stout piece of copper wire was flattened out, and the other end was attached to a glass tube to insulate the copper. The flattened end stood in the path of the jet of water which struck one of the flat surfaces perpendicularly; the distance between the part of the copper which was struck by the water, and the end of the brass jet of the fountain, was 12 inches. The copper wire and the fountain both communicated with the single-leaf electrometer. In the experiments to be described, the water should not be discharged from under a much lower pressure than 3 or 4 atmospheres, and a higher pressure is better.

106. The fountain being now opened and the water consequently dashed against the copper plane, the electrometer was electrified negatively; the charge was feeble, and could scarcely hold the gold-leaf midway between the two plates. The surface of the copper on which the water struck, was cleaned with sand-paper, but the electrical effects did not vary.

107. The long arm of the tin pipe was placed horizontally with the short arm pointing to the zenith, and without disturbing the former arrangement, the long arm of the tin pipe was brought before the jet of the fountain, so that the stream of water might pass axially along it; and the flattened end of the copper wire occupied about the axis of the shorter limb of the tin pipe. The fountain, the tin pipe and copper wire were united to the single-leaf electrometer. The fountain now being opened, plenty of positive electricity went to the electrometer, which must have been collected by the tin pipe

from the mist. The gold-leaf readily struck the conducting plate.

108. The copper wire was removed from the tin pipe, the water consequently now struck against the inside of the short arm of the tin pipe, the fountain and tin pipe were connected with the single-leaf electrometer; the positive electricity remained as before.

109. The jet of water was made to strike against the outside of the tin pipe, everything else as in the last experiment. The electricity was now negative, and rather stronger than in the similar experiment with the copper wire.

110. The arrangement (107.) was restored with this exception, that hot water was placed in the fountain, which was then made to boil by a spirit-lamp; the air was then thrown in until the resistance offered to the piston of the syringe was about the same as with cold water. When the fountain was opened, the positive electricity was perhaps three times more abundant than before.

111. The tin pipe was removed, giving the same arrangement as (105.); the negative electricity remained as with cold water.

112. In the foregoing experiments, in which cold water was used, care was taken to have it as near the temperature of the air of the room as it could be well brought. When the discharged water was about 9° F. colder than the air in which the experiments were conducted, the quantity of electricity remained sensibly the same as with water of the temperature of the air.

113. In these experiments, the apparent excess of positive electricity must have been generated after the water left the metal against which it had been dashed and reduced to a great extent to mist. The similarity between these experiments with the fountain, and that species of electrical excitation which for the sake of distinction I call for the present the electricity of condensation, is very perfect.

114. A gun-metal elbow-piece was screwed into the Armstrong's condenser, which was dry, and the brass jet (40.) by its appropriate connecting pieces was screwed into the elbow-piece and made an angle of about 55° with the horizon. When the cock of the boiler was partially opened so as to allow steam to escape with sufficient force, the boiler became strongly positive; the brass jet was now unscrewed from the connecting piece, the steam fully turned on for a few seconds, shut off, the brass jet restored, then the steam being allowed to escape as in the first instance, it was found to leave the boiler neutral for about the first half-minute. When the water was

not blown out of the steam passages, it took about 8 seconds from the time the steam was first turned on to make the gold-leaf strike the conducting plate of the electrometer, the steam being as nearly as possible of the same pressure at the jet in both cases. These results were constantly obtained; from which I conclude, that water as well as steam is necessary to render the boiler positive. It is perhaps proper to remark, that the elbow-piece used in this experiment was not made for the purpose, but to receive an Armstrong's jet. Also I have succeeded in producing the foregoing electrical effect without the intervention of the elbow-piece.

115. Water was placed in the condenser, and the Armstrong's jet screwed into its place in the condenser; the steam easily rendered the boiler positive, but this jet was by no means a good exciter of this species of electricity. I have also made the boiler positive with discharging passages of lead, pewter, glass, cane, quill, and variously shaped brass orifices; the best was perhaps the cane, which was about $\frac{1}{3}$ inch long.

116. The condenser was dried, and a piece of cane of a length and size sufficient to fill about $\frac{1}{3}$rd of the bore of the pipe of the condenser, which was about 8 inches long and ·55 inch diameter, was well soaked in a solution consisting of 1 hydrate of soda and 12 water, and placed in the pipe of the condenser; then the brass jet was screwed into its place. It was found the boiler could be made positive at the lower pressures as easily as with pure water, although the fluid which escaped felt strongly alkaline. The boiler could not be rendered negative, but was neutral at the higher pressures, as Dr. Faraday had previously discovered.

117. The larger collector (46.) was held in the steam at distances varying from 9 to 24 inches; it received a powerful positive charge of the electricity of condensation; but this effect soon ceased, being dependent, as before observed, on a considerable quantity of moisture escaping with the steam. The boiler was found to be neutral both before and after such experiments. These experiments were made by discharging the steam at various pressures from 6 to perhaps 20 lbs. on the inch. It was necessary occasionally to renew the solution of soda in the pores of the cane.

118. A cylindrical piece of cane divested of its siliceous covering, 6 inches long and $\frac{7}{20}$ inch diameter, was soaked in a concentrated aqueous solution of ammonia, and placed in the pipe of the condenser. The boiler could be made positive as easily as with pure water.

119. The last-mentioned piece of cane was dried and soaked

in a solution of acetate of ammonia, made by adding an excess of the solution of ammonia to a concentrated solution of acetic acid. The cane being again placed in the pipe of the condenser, and the boiler opened, the positive state of the boiler was found unaltered.

120. When two bodies are rubbed together to produce machine electricity, it is not necessary that both of them should be bad conductors, it is enough to have one surface in that condition. If, therefore, the electricity which renders the boiler positive and the steam negative, is produced by the friction of the steam on the wetted surface of the orifice of the jet, then we see why increasing the conducting power of the water does not prevent the production of this electricity. I think this is the only interpretation the experiments will be found to bear.

121. A piece of cane, about the same size as the former pieces, and saturated with turpentine, was placed in the pipe of the condenser. When the cock was opened a little, the boiler was positive; and the positive effect was much stronger than I ever before observed in the similar experiments without turpentine.

122. The steam was now raised to perhaps 15 lbs. on the inch, and the cock fully opened; the larger collector being held as usual in the steam, received a very powerful positive charge as measured by the single-leaf electrometer, and the boiler was at the same time strongly positive to the two-leaved electrometer*. I also rendered the steam and boiler both positive at pressures greatly below 6 lbs. on the inch.

123. A pewter tube, 9 inches long and $\frac{3}{20}$ inch internal diameter, had one end attached to the pipe of the condenser, the brass jet (40.) being screwed into the other end. A slip of cane about 7 inches long was placed in the pewter tube, and so thick as to fill up about $\frac{1}{3}$ the space of the pewter tube in which it lay. The boiler could be rendered positive as when the jet was screwed into the condenser. The pipe of the condenser, connecting pieces, &c. were of course quite clean.

124. A piece of cane similar to that last mentioned was dipped into olive oil and substituted for the former piece in the pewter tube. On allowing the steam to issue, I observed in many experiments a decided diminution in the quantity of positive electricity given to the boiler by the friction of the steam in the jet; in other experiments I could not perceive any effect produced by the oil. The diminished effect was especially remarked after the steam had been issuing for some time, and most of the oil had been thus blown out.

* See Faraday's Researches in Electricity, vol. ii. p. 116, and following.

125. A piece of cane similar to the last was dipped into a solution of common resin in spirit of wine of the consistence of rather thin spirit varnish; the cane was partly dried and used as in the foregoing experiment with oil. The electrical results were similar to those obtained with the oil. I employed the pewter tube in the experiments with oil and resin, not knowing what trouble I might have in thoroughly removing such substances from the pipe of the condenser so as to bring the instrument to its normal state.

126. Having seen reason to conclude that the friction of steam on the wetted surface of the orifice from which it escapes renders that surface positive and the steam negative, it may be about as safely assumed that when water gives up its motion to air, the water becomes positive and the air negative, which I take at present to be the explanation of such experiments as (107.); further, when hot water is used, the specific inductive capacity of the steam-cloud enables the water and air to take a higher charge, just as it did the brass and water in the experiments with the fountain (84, &c.). The experiments (107, 108.) are so similar to those with the jet of steam (56, &c.), that I cannot but recur to the theory before mentioned (65.) as the explanation of the cause of the electricity of condensation. In such experiments as (47, 89.), where no condensation occurs, but a check is only given to the motion of the steam, I suppose the particles of water, which are discharged with the steam in giving up their motion to the steam, and perhaps when they derive their motion from the steam, become positive and the steam negative; part of these particles strike on the tube, and are incorporated with the stratum of moisture, rendering the tube positive, while the remainder take the negative charge from the steam, and in some measure pass out of the tube. Some electricity must also be simultaneously developed by the friction of the steam on the wetted glass.

127. I think it probable that the magnetism of steam, and thermo-electricity generally, may be occasioned by friction, the condensation in the case of steam being only effective in producing friction; for it is not difficult to imagine, that when a current of particles of steam strike against a tube and are condensed, or strike against cold air, that the friction must be of a much more violent nature than when no condensation occurs. I intend to return to this so soon as I have finished some experiments I have in hand bearing on the subject.

128. According to these views, we have that electricity of steam which is so abundantly afforded by Mr. Armstrong's jet, produced by the friction of particles of water against the discharging orifice (Faraday); that electricity produced in such

experiments as (71, &c.), occasioned by the friction of steam on water held by cohesion to the surface of the discharging orifice; and that, called the electricity of condensation, produced by the resistance the particles of water offer to the steam, when the steam gives or takes from them motion.

129. In applying these facts to meteorological phænomena, we have to add wind to condensation to account for electrical developments; the friction between air and water producing the electricity, and cloud by its high specific inductive capacity permitting the charge to be maintained. A sudden abundant rain can only be produced by about as sudden an intermixture of masses of the atmosphere, and hence the connexion which always exists between wind and thunder-showers. The structure of hail shows that it has been much exposed to wind*. In addition to those instances I have already mentioned in which atmospheric electricity may be explained by reference to the experimental facts brought forward, one would expect some electricity to be produced by a fog driving over a wet surface. I would also suggest, that if a mass of ice, water, or vapour should ever enter the atmosphere with great velocity, the steam and friction, together with the coldness and rarity of the air in that region, would be very favourable to the exhibition of electrical meteorous light.

7 Prospect Place, Ball's Pond Road,
May 7, 1850.

LXIV. *Report on the Heat of Combination.* *By* Thomas Andrews, *M.D., F.R.S., M.R.I.A.*†

THERE are few molecular changes in the condition of matter which are not accompanied by the evolution or absorption of heat. The quantity of heat which is thus set free or absorbed, bears always a definite relation to the amount of the mechanical or chemical action, and its determination in each particular case is a problem of considerable interest as affording a measure of the forces in action. If we consider the great number of phænomena, mechanical, electrical and chemical, among which the production of heat forms the only bond of connexion which has hitherto been clearly ascertained, although there may be strong grounds for suspecting them to be only modified forms of the action of the same force, the importance of investigations of this kind to the future progress of physical science will become at once apparent.

The object of the present Report is to give a general view of the actual state of knowledge on the subject of thermo-chemistry, under which we may conveniently include a description of the thermal

* See Dr. Waller's Observations on Hail, Phil. Mag., vol. xxx. p. 166.

† From the Report of British Association for 1849.

effects that occur in chemical actions of every kind. A few new experiments will be described in their proper places. These will be given in some detail, but when referring to experiments already published, all numerical quantities will, as far as possible, be avoided.

Before entering upon the consideration of chemical combinations and decompositions properly so called, it may be useful briefly to refer to the thermal changes which accompany solution. The earlier experiments on this subject having been made solely with the object of discovering frigorific mixtures, do not furnish quantitative measures of any scientific value. But of late years the inquiry has been pursued in a more useful way by GayLussac, Thomson, Karsten, Chodnew and Graham. The salts examined have been chiefly the soluble sulphates, nitrates and chlorides, and the solvents pure water and saline and acid solutions. The principal results of these investigations I have endeavoured to express in the following propositions:—

1. The solution of a crystallized salt in water is always accompanied by an absorption of heat.

2. If equal weights of the same salt be dissolved in succession in the same liquid, the heat absorbed will be less on each new addition of salt.

3. The heat absorbed by the solution of a salt in water holding other salts dissolved, is generally less than that absorbed by its solution in pure water.

4. The heat absorbed by the solution of a salt in the dilute mineral acids, is generally greater than that absorbed by its solution in water.

As the subject is of great extent and the inquiry has hitherto embraced only a small number of cases of solution, it is not unlikely that some of these conclusions will require hereafter to be modified. From some experiments by Graham on the solution of salts belonging to certain isomorphous groups, there is reason to suspect the existence of a connexion between isomorphism and the absorption of heat in solution.

The foregoing remarks apply only to the solution of crystallized salts. If, however, we take a salt which crystallizes with water and make it anhydrous before solution, the thermal results will be altogether different. The anhydrous salt, when added to an excess of water, will first combine with its ordinary equivalent of water of crystallization, and the new compound will then dissolve. The change of temperature observed is therefore a complex quantity arising from the heat of combination due to the union of the anhydrous salt with water, and the heat absorbed by the solution of the hydrous salt. From a comparison of the results obtained on dissolving the same salt in the anhydrous and hydrous states, Graham has endeavoured to deduce the amount of heat due to the combination of the dry salt with its water of crystallization. According to his experiments, the sulphates of water, copper and manganese, disengage the same quantity of heat in combining with the first atom of water. The sulphates of magnesia and zinc also disengage equal quantities of heat in their complete hydration. The same simple relation is not however ob-

served to hold between the quantities of heat evolved in the complete hydration of the first set of salts, or in the combination of the second set with the first atom of water. Neither does it apply to the other sulphates of the magnesian series.

None of the experiments hitherto published furnish all the requisite data for calculating with precision the absolute quantities of heat set free or absorbed in these cases of chemical action. The weights of the water and of the salt are given, and sometimes the weight and form of the vessel, and the material of which it is composed; but these data are not sufficient to enable us to deduce the true numbers from the observed increments or decrements of temperature. Knowing the weight and composition of the containing vessel, we may, it is true, calculate its thermal value in water. But other corrections, such as those for the heating and cooling influence of the surrounding air, can only be ascertained by special experiments performed under similar conditions to the original observations. Neither have any experiments of sufficient accuracy been made to determine the specific heats of the solutions formed.

To complete an investigation which would furnish all these elements, would be a work of very great labour, and will probably scarcely be undertaken till our instruments and means of observation are greatly improved. As a first step to such an inquiry, I may here describe a few preliminary experiments on the specific heats of some saline solutions, and on the quantities of heat absorbed in the solution of successive portions of the same salt.

To obtain results approaching to accuracy in experiments on the specific heats of saline solutions is extremely difficult, as the errors of experiment are often of nearly the same order of magnitude as the whole differences to be observed. The corrections for the cooling and heating action of the air and for the effects of radiation, cannot be estimated with any certainty by the application of general formulas founded on experiments made at a different time*; and the most careful examination of the calibre of the thermometer tube will fail to render different parts of the scale accurately comparable with one another to a five-hundredth part. The general method pursued in the determination of the following specific heats was the same which I described some years ago†; but to avoid the uncertainties just referred to, alternate experiments were made with pure water and with the solution, under conditions as nearly as possible identical, and these were repeated till accurate means were obtained. By this mode of operating, a very great degree of precision may be given to experiments of this kind.

The only salts whose solutions have yet been examined are the nitrate of potash, the nitrate of soda and the chloride of sodium.

* If the vessel be uncovered, changes in the hygrometric state of the atmosphere produce a very marked influence on the rate of cooling, when the excess of temperature above the air amounts only to a few degrees; and even in a close apartment the increased agitation of the air on a windy day sensibly increases the rate of cooling.

† Philosophical Transactions for 1844, p. 34.

They were all chemically pure. The density of each solution compared with water at the same temperature was also determined.

The first solution of nitrate of potash contained for every 100 parts of water 25·29 parts of the salt. The thermal values of the thermometer with large reservoir described in the paper already referred to, in terms of this solution and of water, were found in alternate experiments to be—

	Solution I.	Water.
	5044	4095
	5047	4107
	5050	4116
Mean....	5047	4106

The temperature of the air during these experiments varied only from 18° C. to 18°·5 C.

The second and third solutions contained respectively 12·645 and 6·322 parts of nitrate of potash for 100 parts water. Air from 18°·5 to 18°·9.

	Solution II.	Solution III.	Water.
	4600	4393	4118
	4620	4387	4105
	4605	4385	4108
	4610		
Mean....	4610	4387	4110

From these data the specific heats of these solutions at the temperatures at which the experiments were performed, as compared with water at the same temperatures, may be easily computed. I have given them in the following table, as also the specific gravities of the liquids.

	I.	II.	III.
Specific heat	0·8135	0·8915	0·9369
Specific gravity	1·1368	1·0728	1·0382

The solutions of nitrate of soda contained 42·49, 21·245 and 10·622 parts respectively of nitrate of soda for 100 parts of water. The temperature of the air ranged from 17°·5 to 18°·8 during these experiments.

	Solution I.	Water.	Solution II.	Water.	Solution III.	Water.
	5261	4107	4775	4116	4499	4116
	5234	5117	4782	4098	4498	4098
	5247	4119	4787	4100	4488	4100
Mean....	5247	4114	4781	4105	4495	4105

	I.	II.	III.
Specific heat	0·7838	0·8585	0·9131
Specific gravity	1·2272	1·1256	1·0652

Of chloride of sodium two solutions were examined, the first containing 29·215, the second 14·607 chloride of sodium for 100 water. The air was nearly steady between 17°·9 and 18°.

	Solution I.	Water.	Solution II.	Water.
	5107	4111	4740	4111
	5127	4106	4733	4106
	5128		4731	
Mean....	5121	4108	4735	4108

	I.	II.
Specific heat............	0·8018	0·8671
Specific gravity...........	1·1724	1·0942

It may be not uninteresting to compare these numbers with those deduced by calculation from the specific heats of the salts in the dry state. The latter have been made the subject of experiment by Avogadro and Regnault, but their results do not agree well with each other. I have adopted Regnault's numbers in my calculations.

Solution.		Specific heat by experiment.	Mean spec. heat of dry salt and water.
Nitrate of potash	1.	0·8135	0·8463
" "	2.	0·8915	0·9145
" "	3.	0·9369	0·9566
Nitrate of soda	1.	0·7838	0·7847
" "	2.	0·8585	0·8736
" "	3.	0·9131	0·9307
Chloride of sodium	1.	0·8018	0·8224
" "	2.	0·8671	0·9000

It is obvious that the specific heat of the solution is, in every instance, less than the mean of the specific heats of its component parts, and that serious errors would be committed, if we should attempt to calculate on this principle of the thermal values of solutions which may be formed in the course of our experiments.

I have made a short series of experiments on the quantities of heat absorbed during the solution of nitrate of soda and of nitrate of potash, when added in successive portions to the same liquid. The results fully confirm those previously obtained by Graham, but as the experiments were only preliminary trials to a more extended investigation, it is not necessary to describe them in detail. I may briefly state, that on dissolving 12·22 grammes of nitrate of soda in 250 grammes of water and repeating the experiment with each new solution, till the water was nearly saturated, the following decrements of temperature were found:—

1.	2°·80 C.	6.	1°·60 C.
2.	2·43	7.	1·47
3.	2·11	8.	1·39
4.	1·89	9.	1·33
5.	1·75	10.	1·27
		11.	1·21

By the aid of the specific heats already determined, and knowing the thermal value of the vessel in which the experiments were performed (4·3 grms.), I have calculated for experiments 1, 4 and 9 the

following numbers, which express the degrees Centigrade through which one part of water would be raised by the heat absorbed in the solution of one part of the salt.

1.	2.	3.
590	407	309

On dissolving 7·99 grms. nitrate of potash in 250 grms. water and repeating the operation as before, the successive decrements of temperature observed were,—

1.	2°·65 C.	5.	2°·06 C.
2.	2·49	6.	1·97
3.	2·34	7.	1·87
4.	2·22	8.	1·75

Combination of Sulphuric Acid with Water.—In an elaborate memoir on thermo-chemistry, which was published in Poggendorff's 'Annalen,' Hess made the first systematic attempt to reduce the quantities of heat disengaged in the formation of the hydrates of sulphuric acid to definite laws. His experiments were made by two distinct methods, which however did not give exactly the same results. In the first or indirect method of operating, equivalent quantities of SO_3, SO_3 HO, SO_3 2HO, &c., were respectively mixed with a large excess of water and the increments of temperature observed in each case. The difference between the increments observed on mixing any two compounds with water, was assumed to correspond to the heat due to the combination of the first compound with the number of equivalents of water necessary to convert it into the second. Thus, if SO_3 HO added to x HO gave a units of heat, and SO_3 3HO added to the same x HO gave b units, $a-b$ was supposed to represent the number of units which would be obtained on combining SO_3 HO and 2HO. In the second, or direct method, each compound was combined with the quantity of water exactly required to convert it into the succeeding compound, and the heat measured by observing the increment of temperature of a determinate quantity of water surrounding the vessel in which the combination took place. These experiments have since been repeated by Graham, Abria, and Fabre and Silbermann, but their results do not generally agree with the statements of Hess.

The fundamental principle laid down by the latter is, that there exists a simple relation between the numbers which express the quantities of heat set free in the formation of the successive hydrates of sulphuric acid. If we designate by $2a$ the heat disengaged in the combination of SO_3 HO with HO, then, according to Hess, the heat set free in the formation of the other hydrates will be

SO_3+HO	$8a$
$SO_3\,HO+HO$	$2a$
$SO_3\,2HO+HO$	a
$SO_3\,3HO+3HO$	a
$SO_3\,6HO+x HO$	$a.$

In an early part of his memoir, Hess gives 38·85 for the value of *a*, but this he afterwards changes to 46·94, still maintaining however the accuracy of the ratios. It is difficult to see how this can be correct. The only experiment described by Hess on the combination of the anhydrous acid with water gave the number 305, which bears to 46·94, not the ratio of 8 : 2, but nearly that of 6·5 to 2. Abria obtained a still lower number for the combination SO_3 with HO. There can therefore be little doubt, if the experiments may be relied on, that the first ratio is too high. It remains to be seen how far the others have been confirmed by subsequent investigations.

The multipliers of *a* for the three latter combinations given in the preceding table are, according to Graham's experiments, 0·72, 1·35 and 1·18. These numbers agree with Hess's statement only so far as to indicate that the heat evolved in the combination of SO_3 HO with HO is nearly the same as that evolved in the combination of SO_3 2HO with 4HO.

The experiments of Abria were performed by the direct method and with a similar apparatus to that employed by Hess. Adopting the views of Hess as to the quantities of heat in the cases of combination being in simple relations to one another, he arrives nevertheless at very different numbers for the ratios. In the next table I have given Abria's theoretical whole numbers, as also the exact numbers which result from his experiments.

	Theory.	Experiment.
SO_3 + HO	$6a$	$6{\cdot}02a$
SO_3 HO + HO	$2a$	$2{\cdot}00a$
SO_3 2HO + HO	a	$0{\cdot}95a$
SO_3 3HO + HO	$\frac{4}{8}a$	$0{\cdot}57a$
SO_3 4HO + HO	$\frac{3}{8}a$	$0{\cdot}35a$
SO_3 5HO + HO	$\frac{2}{8}a$	$0{\cdot}22a$

In the three latter cases, the simple relations in the second column are scarcely borne out by the experimental numbers. The only agreement with the ratios given by Hess is in the combination SO_3 2HO with HO, which, according to both experimenters, sets free exactly half as much heat as the combination SO_3HO with HO. The value of *a*, given by Abria, is 39·33.

The latest experiments on this subject are those of Fabre and Silbermann, from which I have calculated the following multipliers for *a*:—

SO_3 HO + HO...........	$2{\cdot}00a$
SO_3 2HO + HO	$0{\cdot}93a$
SO_3 3HO + HO	$0{\cdot}53a$
SO_3 4HO + HO	$0{\cdot}32a$
SO_3 5HO + HO	$0{\cdot}26a$

Hess has also attempted to express by simple multiple relations the quantities of heat disengaged in the formation of the hydrates of nitric acid, but for the details of his results I must refer to the original memoir.

Combination of Acids and Bases.—In the same memoir Hess de-

scribes an extensive set of experiments on the heat evolved during the union of certain bases with acids of different degrees of concentration. These experiments serve to illustrate the general principle, that in the formation of a chemical compound the heat developed is a constant quantity, being the same in amount, whether the combination takes place directly at one time or indirectly at repeated times. Thus he finds that on neutralizing an aqueous solution of ammonia with sulphuric acid, containing one, two, three and six atoms of water, there is a different development of heat in each case; but by adding to the results found by experiment in the three latter cases the quantities of heat due to the combination of the monohydrated acid, with one, two and five atoms of water respectively, the same number is obtained in each case as in the direct combination of the monohydrated acid itself. This principle is correct, but it is almost self-evident and scarcely required so elaborate a proof.

The bases examined by Hess were potash, soda, ammonia and lime, which he combined in different ways with the sulphuric, nitric and hydrochloric acids. The conclusion at which he arrives is, that the same acid in combining with equivalents of different bases produces the same quantity of heat, but at the same time he expresses some doubt as to the applicability of this principle to all similar cases of combination. Indeed his own experiments with lime and ammonia do not accurately agree with it; I refer particularly to his experiments with ammonia, which, when properly interpreted, appear to me to prove clearly that that base in combining with acids developes less heat than potash or soda, although I am aware that Hess himself has drawn from them a different conclusion.

About the time of the publication of the first part of Hess's memoir, I had completed an investigation of the same subject, but instead of employing strong solutions of the acids and bases, I diluted all the liquids largely with water previous to examining their thermal reactions. In this way I hoped to avoid the complex effects that arise when successive combinations and decompositions of different kinds occur in the same chemical action, and the result fully realized my anticipations. The general conclusion deduced from this investigation may be briefly expressed, by stating that *the heat developed during the union of acids and bases is determined by the base and not by the acid.* The following special laws will be found to comprehend the greater number of cases of chemical action to which the foregoing principle can be made to apply.

1. An equivalent of the same base, combined with different acids, produces *nearly* the same quantity of heat.

2. An equivalent of the same acid, combined with different bases, produces different quantities of heat.

3. When a neutral salt is converted into an acid salt by combining with one or more equivalents of acid, no disengagement of heat occurs.

4. When a double salt is formed by the union of two neutral salts, no disengagement of heat occurs.

5. When a neutral salt is converted into a basic salt, the combination is accompanied by the disengagement of heat.

6. When one and the same base displaces another from any of its neutral combinations, the heat evolved or absorbed is always the same whatever the acid element may be.

As some of the bases (potash, soda, barytes and strontia) form what we may perhaps designate an isothermal group, such bases will develope the same, or nearly the same heat in combining with an acid, and no heat will be developed during their mutual displacements.

These laws are not intended to embrace the thermal changes which occur during the conversion of an anhydrous acid and base into a crystalline compound. The steps by which such a conversion is effected are generally very complicated, and involve successive combinations and decompositions. We cannot combine, at ordinary temperatures, a dry acid and a dry base; and when combination takes place in presence of water, hydrates of the acid and base are first formed, which are afterwards decomposed, and the crystalline salt finally obtained is sometimes anhydrous, sometimes combined with water. To expect simple results where so many different actions must produce each its proper thermal effect, would be altogether vain, and to introduce the consideration of some of these actions without the whole would only render the numbers empirical. In the experiments from which the foregoing laws were deduced, the acids and bases before combination, and the compounds after combination, were as nearly as possible in the same physical state. The only change which occurred was the combination of the acid and base, and the heat evolved must therefore have arisen from the act of combination. Such changes of temperature as are produced by solution are not in any way concerned in producing these thermal effects, as none of the reacting bodies assumed at any time the solid state. The insoluble bases form, it is true, an unavoidable exception to this statement, and in the experiments with them, the results would require to be corrected for the heat due to the change of the base from the solid to the fluid state. As this correction, however, although unknown, must be a constant quantity for the same base, it would not, if applied, interfere with the direct proof of the first law.

In an inquiry of this kind, it is important, while endeavouring to generalize the results of experiment, to point out at the same time the differences which occur in particular cases between those results and the numbers deduced from the theory. In the whole range of the science of heat, scarcely a single general principle has yet been discovered which is strictly in accordance with all the results of experiment; and from the application of improved methods of experimenting, discrepancies of this kind have of late years been found to exist where they had not before been suspected.

In the original experiments from which the first of the foregoing laws was deduced, the mean heat developed by the nitric, phosphoric, arsenic, hydrochloric, hydriodic, boracic, chromic and oxalic acids being 6°·61, the greatest deviation from the mean on either side

amounted only to 0°·15; and a similar remark may be made with respect to the combinations of soda, barytes and ammonia. On the other hand, sulphuric acid disengaged about 0°·7 more than the mean quantity, and the citric, tartaric and succinic acids about 0°·5 less. To ascertain whether these discrepancies depended on the state of dilution of the solutions, I repeated these experiments lately with solutions of only half the strength, but although only half the heat was obtained, similar differences were still found to exist. If, instead of taking just the quantity of sulphuric acid required to neutralize the base, we employ a large excess, the heat given out during combination will be nearly 0°·2 less, which reduces the anomaly presented by this acid to about 0°·5. The sulphurous acid not having been formerly examined, I have lately made some experiments on its thermal relations to the bases, the results of which are very interesting. Although one of the feeblest acids, it agrees almost exactly with sulphuric acid in the heat developed by its combination with potash. In several carefully conducted experiments the increments of temperature did not differ more than 0°·05. Combining this with the fact that acids differing so much in composition and properties as the nitric, boracic and oxalic, also disengage almost exactly the same amount of heat in the act of combination, there will, I conceive, be little hesitation in attributing the deviations already mentioned to the influence of extraneous causes, and in acknowledging the truth of the principle, that the heat of combination depends upon the neutralization or combination of the base, and not upon the nature of the acid by which the base is neutralized. That other causes of change of temperature, of feeble power, do actually exist, may be proved by the following fact. If we add an excess of sulphuric acid to the neutral solution after combination has taken place, a slight fall of temperature, amounting to about 0°·1, will occur; if we make the same experiment with sulphurous acid, an increase of temperature of about equal amount will be observed, while with oxalic acid there will be no thermal change of any kind. Now it is very probable that the same causes which produce these slight thermal effects are in operation during the original combination of the acid and base, and if so, they would introduce anomalies into the quantities of heat then developed.

There is one important condition, which, as far as my investigations extend, requires to be fulfilled in order that the first law may hold good; viz. the acid must have the power of neutralizing the alkaline reaction of the bases. It is for this reason that the hydrocyanic, carbonic and arsenious acids do not develope the same quantity of heat in combining with potash as the other acids. The sparing solubility of the arsenious acid in water prevents an accurate examination of its thermal reactions; but on repeated trials I obtained 0°·25 F., on combining with it the same quantity of potash which under similar conditions gave 0°·34 with nitric acid. Although a considerable excess of arsenious acid was taken, as proved by the fact that further additions produced no new development of heat, the solution still exhibited an alkaline reaction.

The same is also well known to be true of the hydrocyanic and carbonic acids. In the case of bases, such as the oxide of copper, whose salts have all an acid reaction, this criterion will not apply; but the exceptional acids are so few, and their peculiarities so well-marked, that they give rise to little difficulty in the experimental investigation.

The quantities of heat developed by different bases in combining with the same acid are so different, that it is unnecessary to refer particularly to the proofs of the second law. In this case, neutralizing power has no apparent influence on the results, as oxide of silver, which forms salts neutral to test paper with the strongest acids, is one of the feeblest bases if measured by its thermal power. It developes, in fact, little more than one-third of the heat which potash does in combining with the acids.

The more recent experiments of Graham and of Fabre and Silbermann, confirm the accuracy of the facts from which the second and third laws were deduced, that no heat is developed on mixing solutions of neutral salts or of a neutral salt and acid*. It is difficult however to obtain, as Graham has remarked, positive proof of the occurrence of combination, when such solutions are brought into contact. Fabre and Silbermann indeed are of opinion that acid salts cannot exist in the state of solution.

Double Decompositions.—When solutions of two neutral salts are mixed and a precipitate formed from their mutual decomposition, there is always a disengagement of heat, which, though not considerable, is perfectly definite in amount. It does not altogether arise from the components of the precipitate having changed from the fluid to the solid state—as it is not always the same for the same precipitate—but it is chiefly connected with the latent heat of the precipitate. If the latter contains water of crystallization, the heat given out is much greater than when an anhydrous precipitate is formed. Experiments of this kind appear at first view to be extremely simple, but it is often difficult to obtain exact results, from the length of time during which the heat continues to be disengaged, even when the combination is aided by brisk agitation.

The precipitation of the salts of barytes and lead by a soluble sulphate appeared to present favourable conditions for investigation, and accordingly I made an extensive set of experiments with these classes of salts. This is indeed the only part of the inquiry which I have been able to complete. A few other examples of double decomposition will however be noticed.

Chloride of Barium and Sulphate of Magnesia.—Of chloride of barium carefully purified and dried immediately before the experiment at a low red heat, 16·94 grms. were taken in each experiment, equivalent to 19·00 grms. sulphate of barytes. The weight of sulphate of magnesia (dry) was 10·3 grms., which is a little more than

* Slight changes of temperature may however occasionally be detected; but in some cases a development, in others, an absorption of heat occurs. These thermal effects evidently arise from causes altogether distinct from those which produce the combination of acids and bases.

sufficient to decompose completely the chloride of barium. The entire weight of the water employed to dissolve the salts was 234 grms., of which one-third was taken to dissolve the sulphate of magnesia, and two-thirds to dissolve the chloride of barium. The solutions were contained in vessels of thin copper, the smaller of which, when filled with its solution, floated in the larger, and could be rapidly rotated, so as to produce in a short time a perfect equilibrium of temperature throughout the whole apparatus. The thermometer attained a maximum about 8′ after the solutions were mixed. I have elsewhere indicated the precautions to be taken in such experiments, and shall therefore not refer to them here. In the following statements, I have given the temperature of the air, the increment actually observed in Centigrade degrees, and the number of degrees through which 1 grm. of water would be raised by the precipitation of 1 grm. and 1 equiv. (oxygen = 1) of the precipitate. In calculating the latter numbers, all the usual corrections were applied to the observed increments of temperature:—

Temperature of air	18°·3		14°·4
Increments observed	1·95		1·96
Heat for 1 grm. BaO, SO_3	25·4		25·2
Heat for 1 equiv. BaO, SO_3 ..		368·9	

Chloride of Barium and Sulphate of Soda.—The same weight of chloride of barium taken as before, and an equivalent weight of sulphate of soda.

Temperature of air	20°·2		18°·7
Increments observed	1·57		1·55
Heat for 1 grm. BaO, SO_3	20·4		20·1
Heat for 1 equiv. BaO, SO_3 ..		294·5	

Chloride of Barium and Sulphate of Zinc.

Temperature of air	19°·7		19°·6
Increments observed	1·69		1·72
Heat for 1 grm. BaO, SO_3	22·2		22·4
Heat for 1 equiv. BaO, SO_3 ..		325·1	

Chloride of Barium and Protosulphate of Iron.

Temperature of air	18°·8	
Increment observed..............	1·99	
Heat for 1 grm. BaO, SO_3	25·6	
Heat for 1 equiv. BaO, SO_3		373·2

Chloride of Barium and Sulphate of Copper.

Temperature of air	17°·5		17°·6
Increments observed	1·85		1·85
Heat for 1 grm. BaO, SO_3	24·7		24·6
Heat for 1 equiv. BaO, SO_3 ..		359·4	

Chloride of Barium and Sulphate of Ammonia.

Temperature of air	11°·3		11°·1
Increments observed	1·85		1·84
Heat for 1 grm. BaO, SO_3	24·2		24·1
Heat for 1 equiv. BaO, SO_3 ..		352·1	

Nitrate of Barytes and Sulphate of Magnesia.—As the nitrate of barytes is sparingly soluble in water, 10·6 grms. only were taken, which is equivalent to half the quantity of chloride of barium used in the foregoing experiments. The other salts were reduced in the same proportion.

Temperature of air	13°·9		14°·4
Increments observed	0·82		0·82
Heat for 1 grm. BaO, SO_3....	22·2		21·2
Heat for 1 equiv. BaO, SO_3 ..		316·4	

Nitrate of Barytes and Sulphate of Soda.

Temperature of air	14°·4	
Increment observed..............	0·75	
Heat for 1 grm. BaO, SO_3........	20·5	
Heat for 1 equiv. BaO, SO_3		298·9

Nitrate of Barytes and Sulphate of Zinc.

Temperature of air	13°·9		14°·1
Increments observed	0·83		0·83
Heat for 1 grm. BaO, SO_3	22·0		22·0
Heat for 1 equiv. BaO, SO_3 ..		320·7	

Nitrate of Barytes and Sulphate of Copper.

Temperature of air	14°·4		14°·4
Increments observed	0·88		0·91
Heat for 1 grm. BaO, SO_3	23·0		24·5
Heat for 1 equiv. BaO, SO_3 ..		346·2	

The salts of lead were next examined. The precipitation of the sulphate of lead took place with the same facility as that of the sulphate of barytes, the thermometer attaining the maximum in eight minutes.

Acetate of Lead and Sulphate of Magnesia.—The acetate of lead was pure and in crystals, 4·17 grms. precipitated by oxalate of ammonia gave 2·454 grms. oxide of lead, which exactly agrees with the theoretical composition of the salt. In each of the following experiments, 30·80 grms. acetate of lead were taken, corresponding to 24·63 sulphate of lead:—

Temperature of air	12°·7		12°·3
Increments observed	1·01		0·97
Heat for 1 grm. PbO, SO_3	9·9		9·9
Heat for 1 equiv. PbO, SO_3 ..		187·6	

Acetate of Lead and Sulphate of Soda.

Temperature of air	12°·3		12°·2
Increments observed	0·84		0·86
Heat for 1 grm. PbO, SO_3	8·3		8·5
Heat for 1 equiv. PbO, SO_3 ..		159·2	

Acetate of Lead and Sulphate of Zinc.

Temperature of air	12°·3		13°·9
Increments observed	0·41		0·37
Heat for 1 grm. PbO, SO_3	4·1		3·7
Heat for 1 equiv. PbO, SO_3 ..		73·9	

In the last experiment the precipitation was so slow that the thermometer did not attain the highest point for thirteen minutes after the solutions were mixed.

When the salts of lead are precipitated by a neutral oxalate, the heat disengaged is much greater than when they are precipitated by a sulphate. I have not examined in detail the increments of temperature in this class of precipitations, but in one experiment, in which the acetate of lead was precipitated by the oxalate of potash, 36·2 units of heat were obtained for each gramme of oxalate of lead.

In the experiments next to be described, a dilute acid was substituted for one of the neutral solutions.

Chloride of Barium and Sulphuric Acid.—The same quantities of chloride of barium and of water were taken as in the experiments with the neutral sulphates. A slight excess of sulphuric acid was employed to secure complete precipitation.

Temperature of air	17°·8	18°·4	15°·1	9°·8
Increments observed	3·44	3·46	3·38	3·42
Heat for 1 grm. BaO, SO_3..	45·6	45·6	44·0	44·2
Heat for 1 equiv. BaO, SO_3			654·6	

Nitrate of Barytes and Sulphuric Acid.—As in the former experiments, half the usual equivalents only were taken.

Temperature of air	15°·0		15°·3
Increments observed	1·50		1·49
Heat for 1 grm. BaO, SO_3	40·4		39·2
Heat for 1 equiv. BaO, SO_3 ..		580·2	

Acetate of Barytes and Sulphuric Acid.—Half equivalents were taken in this case also.

Temperature of air	12°·3		12°·5
Increments observed	1·90		1·91
Heat for 1 grm. BaO, SO_3	49·5		49·3
Heat for 1 equiv. BaO, SO_3 ..		720·2	

Acetate of Barytes and Oxalic Acid.—11·2 grms. of acetate of barytes and 5·33 grms. oxalic acid taken.

Temperature of air	12°·3	12°·8
Increments observed	1·19	1·19
Heat for 1 grm. BaO, C_2O_3 ..	22·1	21·8
Heat for 1 equiv. BaO, C_2O_3..	309·0	

Acetate of Lead and Sulphuric Acid.—Of the acetate 30·8 grms. taken and an equivalent of the acid.

Temperature of air	14°·9	14°·1
Increments observed	2·84	2·86
Heat for 1 grm. PbO, SO_3	28·0	29·2
Heat for 1 equiv. PbO, SO_3 ..	542·0	

Nitrate of Lead and Sulphuric Acid.—Of nitrate of lead 26·26 grms. taken.

Temperature of air	9°·8	10°·3
Increments observed	1·63	1·66
Heat for 1 grm. PbO, SO_3....	16·3	16·4
Heat for 1 equiv. PbO, SO_3 ..	309·8	

Acetate of Lead and Oxalic Acid.—15·4 grms. of acetate of lead were taken.

Temperature of air....................	9°·8
Increment observed....................	2·12
Heat for 1 grm. PbO, C_2O_3	4·3
Heat for 1 equiv. PbO, C_2O_3	792·9

These experiments can only be regarded as introductory to an extended and interesting subject of inquiry. With such limited data, it would be premature to attempt to draw any general inferences.

Solution of Metals in Nitric Acid.—Every chemist is familiar with the violent action of nitric acid on zinc and copper, and the abundant evolution of gas which accompanies it. But the facility with which the gases may be condensed by the acid solution is probably not so generally known, and when the experiment is made for the first time cannot fail to excite surprise. If a small vessel of thin German glass, of about the capacity of half a fluid ounce, be half-filled with nitric acid of density 1·4, and a slip of zinc be suspended in the upper part so as not to touch the acid, the flask hermetically sealed, and finally inverted while surrounded with cold water, a very violent action will occur, but without bursting the vessel. Having ascertained these facts, there was little difficulty in measuring the heat disengaged during the solution of the metals in nitric acid. The metal was weighed in a glass tube open at one end, which was introduced into a thin glass vessel containing nitric acid of specific gravity 1·4. The latter was then carefully closed and introduced into a copper vessel filled with water, and suspended in a metallic cylinder which was capable of rotation. On inverting the apparatus, the metal and acid came into contact, and the solution was completed in a few seconds. The rotation was afterwards continued for five minutes, which was sufficient to diffuse the heat disengaged through every part of the calorimeter.

Solution of Zinc in Nitric Acid.

	I.	II.	III.	IV.
Temperature of air	4°·5	6°·2	8°·0	5°·8
Increment found ..	2·66	2·78	2·83	2·71
Increment corrected	2·65	2·77	2·82	2·71
Weight of zinc	0·587 grm.	0·600 grm.	0·615 grm.	0·604 grm.
Weight of water ..	294·8	284·4	289·3	294·6
Value of acid......	7·4	6·9	6·5	6·6
Value of vessels....	14·3	14·3	14·3	14·3
Heat of combination	1429	1411	1422	1420

Hence we have for the heat disengaged during the solution in nitric acid of—

1 grm. zinc	1420
1 equiv. zinc	5857

Solution of Copper in Nitric Acid.

	I.	II.	III.	IV.
Temperature of air..	8°·9	6°·8	7°·8	8°·5
Increment found	2·56	2·58	2·58	2·57
Increment corrected ..	2·55	2·56	2·57	2·56
Weight of copper	1·202 grm.	1·204 grm.	1·206 grm.	1·213 grm.
Weight of water ..	274·2	273·2	273·3	275·4
Value of acid	14·5	16·8	15·6	15·5
Value of vessels ..	16·8	16·8	16·8	16·8
Heat of combination	648	652	651	650

We have therefore for the heat disengaged during the solution in nitric acid of—

1 grm. copper	650
1 equiv. copper	2578

I made several attempts to determine the amount of heat disengaged in the solution of iron in nitric acid, but although acids of different strengths were employed, I was unable to obtain satisfactory results, as the iron always assumed the passive state before a sufficient quantity was dissolved to raise the temperature of the water in the calorimeter through 1°. Silver, bismuth and other metals were also tried, but the solution did not proceed with sufficient energy.

The numbers 5857 and 2578 obtained above, are very nearly in the same ratio as 5366 and 2394, which, according to my experiments (and their results differ little from those of Dulong), express the quantities of heat set free by the combustion of zinc and copper in oxygen gas. This shows clearly that the oxidation of the metals is the principal cause of the heat produced during their solution in nitric acid. Other causes of thermal change however exist, which must exercise a considerable influence. Such are the combinations of the oxide with the nitric acid, the separation of the elements of a portion of the nitric acid during the solution, and the condensation of the oxygen gas during the combustion. From these and other

circumstances, it is not unlikely that the numbers expressing the quantities of heat disengaged in these reactions will not be found in all other cases to be so nearly in the same ratio as in the foregoing examples; but it may be presumed that the general results will be the same, and that those metals which produce a greater amount of heat by their combustion in oxygen will also produce a greater amount of heat when dissolving in nitric acid.

The heat produced by the solution of copper in nitromuriatic acid is, according to the result of a single trial, about $\frac{1}{7}$th less than that produced by its solution in nitric acid.

Metallic substitutions.—I have lately treated this part of the subject at so great length in a paper published in the Philosophical Transactions, that I shall here only transcribe the general result of the investigation. It is thus expressed:—"When an equivalent of one and the same metal replaces another in a solution of any of its salts of the same order, the heat developed is always the same; but a change in either of the metals produces a different development of heat." This is evidently an analogous law to that already stated for the thermal changes which accompany basic substitutions. The numerical results are however entirely different in their details.

Combustions in Oxygen Gas.—Since the time when Lavoisier published his celebrated experiments on the heat produced by combustion, the subject has frequently engaged the attention of chemists. But few results were obtained of any scientific value, till the posthumous publication of Dulong's valuable researches, which have formed the basis of all subsequent inquiries. More recently, Grassi and Fabre and Silbermann have examined the same subject, and I have myself lately published a set of experiments upon it, which were made some years ago. With the exception of some of Grassi's results, the numbers obtained by the different experimenters agree very nearly with each other, and we may therefore consider the quantities of heat developed by the combination of oxygen with the more important simple bodies and with some of their compounds to be determined with considerable precision. Fabre and Silbermann have also examined the combustion of carbon in the protoxide of nitrogen. A tabular view of nearly all the numerical results hitherto obtained, will be found in the edition of Gmelin's Hand-book of Chemistry recently published by the Cavendish Society. I shall here therefore confine myself to a few general observations.

The following bodies in their ordinary physical states, viz. hydrogen, carbonic oxide, cyanogen, iron, tin and antimony, disengage nearly the same amount of heat in combining with an equal volume of oxygen. The numbers which express the heat of combination in these cases do not in fact differ from one another more than $\frac{1}{40}$th part of the whole quantity,—a difference which is nearly within the limit of the errors of experiment. This observation applies only to the quantities of heat actually obtained by experiment. But if we apply corrections for the heat due to the changes of physical state which occur in some of these reactions, the same agreement will no longer be observed. Thus in the combustion of car-

bonic oxide, the resulting compound is obtained in the gaseous state, while in the combustion of hydrogen it is condensed during the course of the experiment into a liquid; and if, from the entire quantity of heat evolved in the latter case, we deduct that arising from the condensation of the vapour of water, the result will no longer agree with the quantity of heat obtained in the former case. Protoxide of tin may probably be added to the foregoing list, and perhaps also phosphorus, which disengages however a little more heat than the other bodies.

Sulphur, copper and the protoxide of copper, disengage, during their combustion in oxygen gas, a little more than half the quantity of heat evolved by the preceding class of bodies. Carbon occupies an intermediate position, while zinc gives out a larger quantity of heat than any of the bodies already enumerated; and potassium a still larger quantity than zinc. The combustion of a large number of carbo-hydrogens, alcohols, æthers and organic acids has been examined by Fabre and Silbermann. Their results prove the opinion to be erroneous, that if we subtract the oxygen in the form of water, the remaining elements give the same amount of heat as in the free state.

In the reduction of oxide of iron by hydrogen gas, no perceptible evolution of heat occurs, while in the reduction of the oxide of copper by the same gas, it is well known that ignition takes place, unless the experiment is conducted very slowly. These phænomena are at once explained by the fact, that in combining with oxygen, hydrogen gas disengages nearly the same quantity of heat as iron, and twice as much heat as copper.

Fabre and Silbermann have observed that the heat of combustion is influenced to a considerable extent by the physical state in which the combustible exists before combination. According to their experiments, carbon in the form of the diamond disengages 7824 units of heat during its combustion in oxygen gas; in the form of graphite 7778 units; and in that of wood-charcoal 8080 units. According to my own experiments and those of Despretz, the combustion of wood-charcoal produces only about 7900 units. Fabre and Silbermann have also supposed that they were able to detect differences in the quantities of heat disengaged by sulphur in its different allotropic states. The same chemists have also made the remarkable observation, that a much larger quantity of heat is evolved by the combustion of carbon in the protoxide of nitrogen than in oxygen gas. From this it should follow that in the separation of the elements of the protoxide of nitrogen, heat would be set free. Accordingly, by passing the protoxide of nitrogen through a platina tube heated to redness by burning charcoal in a suitable apparatus, it was found that a larger quantity of heat was actually evolved than could be accounted for by the weight of charcoal burned.

Combustions in Chlorine Gas.—Some years ago, I published the results of an investigation on the quantities of heat evolved in the combination of zinc and iron with chlorine, bromine and iodine;

and I have lately given an account of a set of experiments on the combustion of potassium, tin, antimony, mercury, phosphorus and copper in chlorine gas. So far as I am aware, the only other experiments on this subject are those described by M. Abria on the combustion of hydrogen and phosphorus in chlorine. From a comparison of the results, it appears that in several cases the quantities of heat evolved during the combustion of the same metal in oxygen and chlorine are nearly the same. This observation applies particularly to the cases of iron, tin and antimony. Zinc however disengages a greater quantity of heat with chlorine (6309 units) than with oxygen (5366 units), and copper nearly twice as much (3805 and 2394 units). Phosphorus, on the contrary, gives less heat with chlorine than with oxygen (2683 and 4509 units). On comparing the quantities of heat disengaged by different bodies in combining with the same volume of chlorine, it will be found that potassium disengages a larger amount of heat than any other body hitherto examined, twice as much as zinc, and nearly four times as much as tin, antimony or copper.

Combinations of Bromine and Iodine.—The heat disengaged by the same body in combining with bromine is less than with chlorine, and with iodine less than with bromine. The greater development of heat in the case of chlorine is at least partly due to that element being in the gaseous state before combination. In some early experiments, I observed that the quantities of heat developed on converting equivalent solutions of the sesquichloride, sesquibromide and sesquiiodide of iron into the corresponding proto-compounds were equal. When a solution of protochloride of iron is converted into sesquichloride by agitation with chlorine gas, a definite disengagement of heat occurs, as also in the formation of the sesquibromide of iron by the combination of the protobromide and bromine; but in the corresponding reaction between the protoiodide of iron and iodine, no change of temperature can be observed.

LXV. *On the Connexion of* Pope Gerbert *with the Geometry of* Boethius. *By* George Sloane, *Esq.**

IN the editions of Boethius's collective works we find a translation of the first four books of Euclid, or rather of the propositions or enunciations alone. This treatise is divided into two books, both of which purport to be a translation of Euclid, although in fact the first only is such, the second being for the most part a collection of problems in mensuration†.

* We avail ourselves of the permission of the Philological Society to transfer this Paper to our pages from the Journal of their Proceedings, now extending to four very interesting Volumes: we are also indebted to the Author for the additions and corrections with which he has favoured us.

† T. ii. p. 1487–1546. ed. Basil, 1570. Except in one or two instances, which will be readily distinguished, the references in this paper are to the pages and lines of the new edition of the Agrimensors, 'Gromatici Veteres ex recens. C. Lachmanni,' Berol., 1848.

The so-called translation is followed by a kind of supplement or appendix, which in the printed editions bears the title of *Boethii liber de Geometria,* but in the MSS. of *Demonstratio Artis Geometricæ.* With the exception of a kind of catechism of geometry and some arithmetical observations, which seem to be nothing more than confused extracts from the Arithmetic of Boethius, it contains scarcely anything but fragments from Varro, Seneca, and the Agrimensors. It begins with an introduction on the origin and value of geometry, part of which is to be found in the 'Outlines of Geometry and Astronomy' of Cassiodorus, the friend and contemporary of Boethius, and the rest is, in the opinion of Blume, a free imitation of a passage in Agenus Urbicus*. This introduction is followed by a collection of extracts from Frontinus, Balbus, Hyginus, and the Libri Coloniarum, on the qualitates agrorum, the controversiæ and the limites (p. 395–403); to which are subjoined lists of nomina Agrimensorum and of lapides finales (p. 403–406).

If we turn from the printed editions to the MSS. of the Geometry, we shall find that they differ exceedingly in their contents, as well from the editions as from one another. In the library of Berne, for instance, there are two MSS. of the Geometry, divided into five books, the first two of which correspond to the appendix, the third and fourth to the first, and the fifth to the last of the printed copies. In the older of these MSS.† the matter contained from p. 1544 mid., to the end is wanting; and between the fourth and fifth books is inserted a piece with the title *Altercatio geometricorum de figuris numeris et mensuris* (p. 407 *seq.*): the fifth, besides being fuller than the editions, contains a fragment, De Mensuris et Jugeribus, which is expressly ascribed to Frontinus, but which is partly taken from Columella (v. 1–3), and partly from the fragment De Jugeribus Metiundis (p. 354).

The second Berne MS. has all that is contained in the other, and in very nearly the same order. It has, in addition, Frontinus de Agrorum Qualitate, with the commentary of Agenus Urbicus (p. 1–8); an extract from Hyginus de Limitibus Constituendis (p. 182–191); and a fragment of Censorinus de Geometria‡.

* "Bei aller Verschiedenheiten im Einzelen, doch in Gedanken und Wendungen einer Stelle des Pseudosimplicius verwandt ist, so dass man sie als eine freie Imitation des Leztern bezeichnen könnte." Blume, Ueber die Handschriften der Agrimensoren, in Rhein. Mus. für Jurispr. vii. p. 229. The two related passages are p. 64, 24—65, 14, and 394, 11—395, 14. I confess I can find no similarity in the two, beyond both containing the praise of geometry.

† The contents of this MS., which is of the 10th century, are minutely described by Sinner, Catalogus Codd. MSS. Bibl. Bernensis, p. 292. The title given to the book in the MSS. is 'Boetii libri Artis Geometriæ et Aritmeticæ numero V ab Euclide translati de Græco in Latinum.'

‡ Sinner, *l. c.* p. 292. In the library of Trinity College, Cambridge, there is a MS. of Boethius's Geometry, the contents of which are very similar to, if not identical with, those of the second Berne MS. The loss of some papers prevents me from giving a more detailed account of it. It does not agree with any of the MSS., the readings of which are given by Lachman, in the order of the Nomina Agrimensorum, unless, indeed, there is, as I suspect to be the case, a misprint as to the order of the Munich MSS. (*m*), with which it agrees in reading *Claudii* and *Augustini.* It is also fuller in the *Nomina Lapidum.* The MS. which is probably of the

There are again other MSS. which do not contain so much as the printed copies. Such are the Harleian, Lansdowne, and Arundel MSS. in the British Museum, none of which have the appendix*.

The Harleian and Arundel MSS. coincide in their contents with the editions down to the beginning of the Demonstratio, or Appendix, that is, nearly the foot of p. 1536. Immediately after the table in that page, there are a few lines which have never been published in the original Latin, and the existence of which was unknown until M. Chasles gave a French translation of a portion, in his 'Aperçu sur l'Histoire de Géométrie,' from a MS. belonging to the town of Chartres†. At the end of this passage the Harleian has the words *epilogus finitur*: and then follows in both this sentence—"Si quis vero de controversiis, et de qualitatibus et nominibus agrorum, deque limitibus, et de statibus controversiarum scire desideret, Julium Frontinum necnon Urbicum Agenum lectitet. Nos vero hæc ad præsens dixisse sufficiat."

Here the Arundel MS. ends, but in the Harleian we find what is a meagre abstract of Balbus, followed by a collection of geometrical and arithmetical problems, which are taken, in part at least, from Nipsus, Epaphroditus and Vitruvius‡.

Such and so varied are the contents of the different MSS. We have now to inquire whether any and what part is to be attributed to their reputed author.

The opinion of Niebuhr on the authorship of this treatise is to be found in the appendix to the first edition of the second volume of his 'History.' "It is absolutely certain," says he, "that the section on the art of marking out boundaries in Boethius's Geometry can never have been written by the learned and talented Consular. It is a confused heap of rubbish, almost worse even than the great compilation. Boethius's Geometry, until the appearance of Pope Gerbert's, was, with Nipsus, Vitruvius and Epaphroditus, the manual of the land surveyors; and by one of them has this addition, which dishonours his name, been surreptitiously introduced; just as the rude ignorance of the copyist, at least of the MS. from which it was printed, has stript the propositions and diagrams of what was most essential§."

Blume agrees with Niebuhr in thinking that the Demonstratio is spurious, but differs from him as to the genuineness of the Euclid.

eleventh century, deserves a closer examination. Five MSS. have been used for the new edition of the Agrimensors, two of which (a and m) apparently do not contain the Euclid, and one (z) has only the two books without the appendix.

* These MSS. are respectively numbered 3595, 842, and 339.

† Mémoires Couronnées de l'Académie de Bruxelles, t. xi. p. 471. The contents of this MS. are fully given by M. Chasles in his 'Catalogue des Manuscrits de la Bibliothèque de Chartres,' No. 142. According to Bethman it is not older than the end of the twelfth century.

‡ Only a part of these problems are published in Lachman's edition (p. 297–301). Some of them were also published from the Arcerian MS. by Hase, in Bredow's 'Epistolæ Parisinæ,' p. 201 *seqq.*, and the whole of them by Schott in his 'Tabulæ Rei Nummariæ Rom. et Græc. (Ant. 1615),' from a MS. in the Cistercian Monastery at Duyn, which had also the 'Musica et Arithmetica' of Boethius. Is the MS. in the public library of Cambridge (Moore 74) similar to this?

§ Hist. of Rome, translated by Walters, vol. ii. p. 557.

For allowing, on the authority of Cassiodorus, that Boethius indeed translated the Elements, he contends that the translation, which now passes under the name of Boethius, must be considered as spurious, inasmuch as in most MSS. it is found mixed up with the Demonstratio, and that consequently both must stand or fall together*.

Although it is impossible to produce any positive proof in support of the common opinion that the translation we possess is the work of Boethius, still there is a certain amount of negative evidence to that effect. It is not disputed that Boethius did translate the Elements. Besides the testimony of Cassiodorus already alluded to, we find Gerbert, in his Geometry, referring to the definition of some elementary terms in geometry given by Boethius, and which are apparently identical with those which we find in the treatise in question†. With this we must combine the fact, that until the restoration of the Elements in their perfect form at the close of the eleventh century by Adelard's translation from the Arabic, there was no work, so far as is known, which professed to be a translation of Euclid, save and except the meagre list of propositions which now goes under the name of Boethius.

There seems to be more force in Niebuhr's assertion, that, though the translation is genuine, we have it only in a mutilated form. From the remarks with which Boethius prefaces the demonstrations of the first three propositions of the first book, we may readily assume that Boethius adopted the opinion of those who considered that Euclid only arranged the propositions, and that the demonstrations were the work of others‡. The admirable literary history of the Elements by Mr. De Morgan, in the 'Dictionary of Classical Biography,' shows how this error may have arisen; and when we find Boethius confounding Euclid the geometer with his namesake the philosopher of Megara—a most portentous error, and one quite inexcusable in him,—we ought not to be surprised if he also adopted the current opinion on the subject, viz. that Theon and not Euclid was the author of the demonstrations.

The only argument against the genuineness of the translation which seems to have any weight, is that derived from the circumstance of

* Rhein. Mus. für Jurispr. B. vii. p. 235. He conjectures that a part of the genuine translation probably survives in the 14th and 15th books of a mathematical work to be found in a palimpsest MS. at Verona, which is evidently allied to the printed translation of the summary of Hypsicles. Whatever grounds there may be for denying the genuineness of the common translation, there can be no doubt that this conjecture is altogether unfounded. For though the Elements consist of fifteen books, it is quite clear, as well from the books themselves as from other testimony, that the two last were not written by Euclid; and there are very good grounds for saying that they are the work of Hypsicles, who cannot have written earlier than the middle of the sixth century, that is, at least five-and-twenty years after the death of Boethius. See Mr. De Morgan's articles on Euclid and Hypsicles in the 'Dict. of Classical Biography.'

† Pez, Thes. Anecdot. Noviss. t. iii. part ii. 9.

‡ P. 1514. See also p. 1487 *in*. The passage in p. 1542 may also be referred to, not indeed as containing the opinion of Boethius himself—for it occurs in the spurious appendix—but as indicating that generally entertained at the time of its composition.

a part of the Demonstratio being inserted in the midst of the Euclid in most of the MSS. The part so interpolated is not any of that continuous whole, if it may be so termed, which we have called the Appendix, but a portion of the Altercatio (p. 407, 1–410, 7), filling nearly two leaves in the Bamberg (*b*), and about one leaf in the Rostock (*r*) MS. of the Demonstratio. A careful examination of the contents of each page of the MSS. will convince any one that Blume has made a stronger assertion than the facts warrant, when he says that the two are completely blended together (ganz und gar vermengt), and will at the same time show us how the confusion probably arose *. Leaving out of consideration the two propositions of the third book, inserted in the Altercatio (p. 408, 3–9), all that we find is, that some few of the following propositions (389, 28–390, 20) are placed at the end of the Altercatio. This may, I think, be readily accounted for by supposing that a leaf of the codex from which our present MSS. are derived, containing the portion in question, had been by some accident transposed out of its proper place, and inserted where we now find it. This transposition may also be accounted for by supposing that the writer of the original MS. having by accident probably overlooked or omitted the matter contained in p. 489, 28 *seq.*, did not discover his mistake till he had got to p. 408, 3, where he inserted the two first of the missing propositions, but then changed his mind and reserved the remainder for the conclusion of the piece he was then engaged about. I say the conclusion, for it is evident that the following part of the Altercatio, from p. 410, 8, does not cohere even with the Euclid †.

That the Demonstratio did not proceed from the pen of Boethius, few persons will be inclined to dispute. Independent of the grounds assigned by Niebuhr and Blume for denying its genuineness, the book itself shows that it is the production of a Christian, and that consequently it cannot have been compiled by the author of the Consolatio ‡.

In order to understand and appreciate Blume's opinion on the origin of the treatise we are considering, it is necessary to say a few words on the classification of the different MSS. of the fragments of the Agrimensors. In the article on these MSS. which we have already had occasion to refer to, and in which everything then known

* The sequence of the matter in the MSS. is 387, 1–22; 388, 20–389, 20; 390, 21–391, 16; 391, 24–392, 17; 407, 1–408, 2; 408, 3–9 (389, 21–27); 408, 10–410, 7; 389, 28–390, 20. In the second Berne MS. the Altercatio is, according to Sinner, interpolated in a different place.

† The conclusion of Euclid (p. 390, 20) is not far from the beginning of p. 15 of the Rostock MS., while p. 410, 8, corresponds with the latter half of the following folio. That the writer was very stupid or very careless, is evident. See for instance the confusion in 385, 21–386, 7; 388; 391, 18–26. The whole of 388, 21–389, 20, is such a confused and unintelligible medley, that it has been altogether omitted in the various editions of Boethius' collective works.

‡ "In quibus locis arbores intactæ stare videntur, in quo loco veteres *errantes* sacrificium faciebant," p. 401, 6. In the passage of the Liber Coloniarum (p. 241, 5) from which this is taken, *errantes* is not to be found. That Boethius was a heathen has been clearly shown by Obbarius, in the introduction to his edition of the Consolatio, Jen. 1843.

and calculated to throw light on the subject has been carefully collected by the learned and able author, Blume divides the different MSS. into four classes:—1, that of which the Arcerian is the representative; 2, the MSS. containing the extracts from the Digest; 3, the MSS. of Nipsus; 4, those of Boethius. In the course of the article he has endeavoured to trace, as far as his data permitted, the history of the several MSS. which pass under review, and particularly of the celebrated Codex Arcerianus, which he identifies with the MSS. said to have been discovered by Thomas Phædrus in the Monastery of Bobbio, in the year 1494, and transferred by him to Rome*. The Arcerian is also considered by him to be the source of the fourth-class MSS., or those containing the treatise attributed to Boethius.†

After insisting that the genuineness of the Euclid is bound up with that of the Demonstratio, Blume goes on to say:—" Rather may Gerbert be considered the compiler of this Appendix. For independently of Gerbert's probable connection with the Arcerian at Bobbio, and without reference to the MS. of the third class, in which Goesius says he found the Epistola ad Celsum ascribed to Gerbert, we must most especially take into consideration a MS. belonging to De Thou, which was used by Rigaltius, and is thus described in the Catalogue of De Thou's library:—'Boetii Musica, Arithmetica, Gerberti Geometria et Rhythmomachia‡.'" It was from this MS. that

* Though it is difficult to deny the extreme probability of this supposition, yet there are difficulties which make the author hesitate. The known connection between John Lasco and the celebrated Erasmus would seem to raise a presumption that the Erasmus whose name appears on the MS. was no other than that great philologist. But this would go far to show that the Arcerian was not the same with the Bobbio MS. The MS. is not mentioned either in the Catalogue of the Bobbio library, printed by Muratori in the third volume of the Antiq. Ital., nor yet in the one compiled in the year 1461, and published by Peyron in his 'Commentatio de Bibliotheca Bobiensi.' In the first-mentioned list, which is as old as the tenth century, we find 'Libros Boetii iii. de Aritmetica et alterum de Astronomia.'

† " Mir scheinen nun schon aller Handschriftern, in welchen der Name des Boethius mit vorkomt, zur Arcerianische Familie zugehören," p. 198. In a subsequent page, after pointing out the supposed resemblance of a part of the introduction to a passage in Agenus Urbicus, he proceeds:—" Das Uebrige schliesst sich dem Arcerianus meist wörtlick, und oft selbst büchstablich in sichtbar corrumpirten Lesarten an: doch steht auch Einiges darunter, was sich sonst teils gar nicht, teils wenigstens nicht in Arcerianus erhalten hat," *ib.* p. 299. Though this is undoubtedly true, still in many places it deserts the Arcerian, and agrees with the Erfurdt MS. which belongs to the third class. See, for instance, 395, 20; 396, 4, 5, 15; 403, 8, 10; 409, 17, 20–25. If p. 27, 12 is to be considered as the original of what we have in Boethius, p. 397, 6 and 409, 6, then the writer must have had a MS. of the third class before him, for in neither of the other two classes is the first-mentioned passage to be found. The definition of measure, which Boethius attributes to Frontinus (p. 415, 11), is in the Jena MS. (a transcript of the Arcerian) given to Balbus; in the Gudian, which belongs to the second class, to Frontinus; and in those of the third class, to Nipsus.

‡ According to Oudin, this MS. came into Colbert's collection, and from thence into the National Library at Paris. (Suppl. in Bellarmin. p. 313.) This leads us to identify De Thou's MS. with the one numbered 7185 in that collection, and which is said in the printed catalogue to have belonged to Peter Pithou and afterwards to Colbert. It seems to be a collection of distinct MSS. bound up together. The Arithmetic of Boethius is of the eleventh century, and the Musica of the fourteenth,

Rigaltius copied what he called the Fragmenta Terminalia, but which is an almost literal extract from the Demonstratio (p. 401, 10–403, 4). He most commonly refers to the second book of Boethius, but on one occasion he expressly mentions the revision of Boethius by Gerbert or some one else. Another proof is, that in a published treatise of Gerbert on Geometry, we meet with at least part of one of the extracts from Hyginus, which are to be found in the second Bernese MS. of Boethius*. Blume however is of opinion that the work in its present form is unworthy of Gerbert also:—"For even Gerbert could not have dealt with the contents of the Arcerian MS. in the awkward and silly way in which the MSS. of the pseudo-Boethius represent their compiler to have done: and a part also of its contents must have been derived from a MS. of the second class with which Gerbert was not acquainted so far as we know." He accordingly conjectures that some person living on this side of the Alps got hold of Gerbert's extracts from the Arcerian, and by the help of these and other similar materials, fabricated the work in question. He observes that all the MSS. of the fourth class appear to have proceeded from Alsace or Flanders, whilst those of the third class, on the contrary, had their origin in Italy: and Gerbert, who was continually moving to and fro between France and Italy, was in those times the best medium of communication on such matters, though his words were often mutilated and misunderstood by his ignorant contemporaries†.

Ingenious and plausible as this hypothesis is, the author is unable to assent to it. It is obviously founded on the double assumption that the Arcerian is the identical MS. found at Bobbio by Inghirami, and that Gerbert having become acquainted with it during his tenure of the abbacy of Bobbio, subsequently communicated a part of its contents to the northern and eastern parts of France. At the time that Blume wrote his article it was universally supposed that Gerbert's connection with Bobbio began as early as the year 969 and did not finally cease till 983‡. The subsequent researches of Hock have established that Gerbert did not become abbot of Bobbio till the year 981 or 982, and that he did not continue so above a year,

while Gerbert's Geometry belongs to the thirteenth. According to Chasles (*l. c.* p. 505 note), there is another copy in the same collection, No. 7377 C. This statement is not confirmed by the catalogue, which describes the MS. as only containing two letters on geometrical subjects, one addressed to Gerbert, and the other written by him, and also a MS. with the title 'Geometria Euclidis interprete Boetio.'

* Pez, *l. c.* 81. Gerbert's work was printed from a single MS. belonging to the Monastery of St. Peter at Salzburg, which is manifestly imperfect at the end. Blume suggests that if other copies were examined, its deficiencies might probably be supplied. The copy in the Arundel collection contains only the first thirteen chapters. The only MS. of Gerbert in England that I have been able to discover, is one of the twelfth century, in Sir Thomas Phillips's collection at Middlehill, No. 4437.

† Later researches have proved that Blume is mistaken in confining the MSS. of Boethius to Flanders and Alsace. Besides the one at Chartres above-mentioned, there is one at Middlehill, which came from Tours. They are to be found at St. Gall, and also in the Laurentian library at Florence (Plut. xxix. cod. 19).

‡ Histoire Littéraire de France, t. vi. p. 559 *seqq.*

during which time he was so engaged with secular affairs, that it was hardly possible for him to have bestowed any attention on the corrupt and almost unintelligible MS. of the Agrimensors*.

If we cannot connect Gerbert with the Arcerian MS. at Bobbio, there are, it seems, no reasonable grounds for saying that he was more intimately acquainted with the writings of the Agrimensors than any other well-educated man of his time, unless such connection can be inferred from the statement of Goesius, that part of the Expositio Mensurarum, which in the Arcerian bears the name of Balbus, and in the MSS. of the second class that of Frontinus, was in his MS. attributed to Gerbert (Goes. in not. p. 142). Goesius goes on to say, that he has made some corrections and additions with the aid of that MS., and he expresses his surprise that Rigalt had not done the same, as he had the same MS. lent to him by Rutgersius. Now this MS. lent to Rigalt was undoubtedly nothing more nor less than a transcript of the Arcerian, made by Nansius†, and consequently Goesius was mistaken so far; but it would be too rash to say that he is mistaken as to what he found in a MS. which he had before him. His words are, "Hæc in manuscriptis adscribi video partim M. J. Nipso, partim etiam, ut est in manuscripto, Domno Gerberto Papæ et Philosopho." He distinguishes between the MS. of Nipsus and that of Gerbert. So far as Nipsus is concerned, the difficulty may be got rid of by supposing that Goesius had one or more MSS. of the third class, in which the preface is ascribed to Nipsus. With respect to Gerbert it is not so easy to give any satisfactory explanation. The only way of accounting for it, which occurs to me, is, that as the matter which in the Arcerian is distributed between Epaphroditus, Vitruvius, and Balbus, is in the third-class MSS. given to Nipsus, and as a great part of it is also to be found in Gerbert, all Goesius meant to say was, that such was the case, and not, as his words would lead us to suppose, that any part of Balbus was expressly ascribed to Gerbert; or perhaps he only meant that there was a substantial resemblance between the account of measures, &c. in Balbus, and in Gerbert ‡.

Independently of the presumption against Gerbert's familiarity

* Gerbert oder Papst Sylvester II. und sein Jahrhundert, von C. F. Hock, pp. 64–67 and 195–199. The narrative of Richerius, who was the scholar of Gerbert, and wrote his history at his request, as to the early career of his master, is, I think, quite conclusive against the common opinion as to the time when he became connected with Bobbio.—Richer. Hist. lib. iii. c. 43 *seq.* in Pertz, Monumenta Germanica Historica, t. iii. b. 616. That he had not much leisure for literary pursuits is proved by his own words:—" Cessimus ergo fortunæ, studiaque nostra, tempore intermissa, animo retenta, repetimus" (Ep. 16). "Disparibus in Bobiense Cænobium meritis præstant laudati viri . . . Gerbertus potissimum ob jura abbatialia vindicata . . . . Gerbertus scientias universas attigit: verum vix ad paucos annos (?) rem Bobiensem moderatus est, juribus potius, quam studiis revocandis intentus."—Peyron, *l. c.* p. xi.

† See Blume, *l. c.* p. 180.

‡ Blume thinks that the name of 'Gerbert' was prefixed, perhaps merely to indicate the possessor of an ancient MS., and not the author of the compilation, *l. c.* p. 227. In the Corrigenda, p. 377, he corrects the mistake he had fallen into, that Gerbert's name was written in the Arcerian.

with the Arcerian suggested, by the examination of his personal history, the Geometry itself furnishes evidence almost amounting to demonstration, that its author was unacquainted with it. The most important, and, in an historical point of view, the most interesting proposition of the mathematical part of the manuscript, so far as its contents are known, is the general formula for any triangle in terms of its sides (p. 301, 11–301, 5)*. Now there is not the slightest hint to be found in any of Gerbert's writings of his acquaintance with this formula; and as we know from his letter to Adelbold†, that his attention had been pointedly directed to the rules then ordinarily used for determining the areas of triangles, it is highly improbable that he should have omitted all mention of it, if it had ever come under his notice. The only rule applicable to all triangles given by him is, substantially, that the area is equal to half the sum of every side multiplied by the perpendicular let fall on it from the opposite vertex‡. On the other hand, the extract from Hyginus (p. 188, 14–190, 12), with which the Geometry ends, has been taken, not from the Arcerian, but from the Gudian or some other MS. of the second class; for not only does it agree with the latter, where it differs materially from the first and third class MSS., but also faithfully copies its peculiar blunders and corruptions§.

The next argument is, that Rigaltius has edited from a MS. of Gerbert's Geometry what is in fact a part of the Demonstratio: and Blume refers to Rigaltius's note in p. 240:—"Gerbertus, sive quis alius Boetii Geometrica sublegit, postquam ad hujusmodi negotia pervenit, de iis sese nihil attingere velle profitetur:" and he then gives the sentence which has been before quoted from the Harleian and Arundel MSS. This certainly creates a difficulty, which, in the absence of more accurate information as to the MS. used by Rigaltius, it is not easy to overcome. It must be observed that this sentence does not occur in the Salzburg MS. of Gerbert; and in the Arundel, which has a fragment of his Geometry, it forms a part of the Boethius, and not of Gerbert.

The last argument is derived from the Geometry of Gerbert containing the identical extract from Hyginus as to the methods of ascertaining the true direction of the meridian by observations of the sun, which we find in some MSS. of Boethius. Though this argument is apparently entitled to greater weight than the rest, yet it

* This formula is found also in the MSS. of Boethius, and has been published from the second Berne, by Venturi, 'Commentari sopra la Storia et le Teorie dell' Ottica,' p. 125. It agrees with the Excerpta Rostochiensia, when this differs from the Arcerian, except in 300, 11, where it has *id est*, a reading peculiar to itself.

† Gerbertus ad Adelboldum de Causa Diversitatis Arcearum in Trigono Equilatero geometrice arithmeticeve exposito, in Pez. l. c. 83.

‡ See the passages in Pez, 31 and 59. In 61, the area of an equilateral triangle is said to be equal to $\frac{a^2-a}{2}$.

§ It is much to be wished that we had some information as to the readings of the Boethian MSS. of this passage. Unfortunately my attention was not directed to this point when examining the Cambridge MS.

is far from conclusive, especially as it is founded upon an assumption, the truth of which, in the writer's opinion, is at least doubtful,— that the part of the Geometry containing the passage in question, is the composition of its reputed author. The most cursory examination of the printed treatise will convince the reader that it could not possibly have emanated, in its present form, from "the wise pope who was the instructor of his age." No man of sense would have been so absurd as to repeat the same matter twice or even oftener in so short a compass, or to insert in the body of his book a second introduction, not materially different from the one prefixed to it. Evidently two distinct treatises, the first of which ends with the thirteenth chapter, have been somehow or another confounded in the manuscript, and both have been published as one entire work, by Pez, who has overlooked the internal indications they contain of having originally been unconnected with one another*. If then we have two separate tracts fortuitously united together, which of the two is to be considered as the work of Gerbert? Unfortunately we have no weighty, much less decisive evidence on this point, and the only, or at least principal reason, which with our present scanty data can be urged in favour of the first and shorter of the two, is, that it is the one which bears his name, not only in the Salzburg, but also in the Arundel MS., which is apparently derived from some other source than the former.

The writer is inclined to go a step further, and ask, Is there any evidence that Gerbert ever wrote a work on Geometry, or have we any surer grounds for asserting that either of the two treatises which bear his name was actually written by him, than we have for attributing the work 'De Divisione Numerorum,' which we know to have been composed by him, to Beda†, viz., that in some MSS. his name is attached to it? Beda, Alcuin, and Gerbert were the representatives of the learning of their respective centuries; and to each was ascribed indiscriminately every work of merit, the writer of which was unknown or forgotten.

Granting however that Gerbert became acquainted with the Arcerian at Bobbio, still that fact is far from establishing the conclusion attempted to be drawn from it, as there is reason for believing that

* This opinion seems to receive some confirmation from the circumstance that the Arundel MS. has only the first thirteen chapters, in other words, the first treatise. At Chartres there is a MS. (No. 173), which only has chapters 14–40. The Arundel shows how the two works probably came to be blended into it. The concluding words of Gerbert are immediately followed by the opening sentence of Boethius, as this is in like manner succeeded by another treatise on geometry or mensuration, without the slightest indication that all three do not form one continuous whole.

† The 'Liber ad Grammaticum,' which Richerius (p. 618) says was written by Gerbert, as a companion or guide to the use of the Abacus invented by him, has been printed by M. Chasles, in the Comptes Rendus of the Académie Royale des Sciences, T. xvi., and is the same tract with that published in Beda's works with the title 'De Divisione Numerorum,' Bed. Opera, t. i. 159, ed. Bas. The treatise of Hermannus Contractus 'De Utilitatibus Astrolabii,' which has also been published by Pez, from the same Salzburg MS. is attributed to Gerbert in two MSS. Chasles, Catalogue, p. 44.

the mathematical part of the Arcerian was known long before Gerbert's time. We find, for instance, a part of the problems attributed to Nipsus, Epaphroditus and Vitruvius, in the Propositiones Arithmeticæ, said to be by Beda, but which was probably the work of Alcuin*.

In conclusion we may observe that, in the library of St. Gall there is an old MS. of which the following account is given by Haenel:—"830. Boetius in perihermenias, geometriam, de differentiis, divisionibus, cognatione, syllogismis, topica Ciceronis, Ekkehardi IV. notæ marginales, versus. Cod. membr. optimus, eadem manu scriptus in pergameno solido†." The age of the MS. is not mentioned, but as it contains marginal notes by Ekkehardus IV., it cannot be later than the close of the tenth or the beginning of the following century‡. The oldest of the Berne MSS. belongs, as has been already stated, to the tenth century; and the other, which came from Strasburg, was written in 1004. Here then we have three MSS. almost coeval with Gerbert, and the most modern of which must have been written about twenty-five years after he became abbot of Bobbio, in which the work is attributed to Boethius: and one of which was perused and annotated by the pupil of Notker, the friend of Gerbert, and probably—for he also belonged to St. Gall—by Notker himself. It is hardly possible to conceive that a new forgery, the materials for which are supposed to have been partially derived either from Gerbert, or taken from his work, could in this short space of time have been palmed upon the world as the work of Boethius.

* Bedæ Opera, i. 133. It is printed in the Ratisbon edition of Alcuin (t. ii. p. 442), from a MS. belonging to the Monastery of Richenau, in which it bore the name of Alcuin. In the library of Valenciennes there is a MS. of the tenth century, which formerly belonged to the Monastery of St. Amand or Elnon, and which contains the Podismus (p. 296 *seq.*), but whether it is derived from a first or third class MS. I am unable to say. It is described in Pertz, Archiv der Gesellschaft für D. Gesch. viii. 440.

† Haenel, Catal. MSS. 712. There is another MS. of the ninth century at St. Gall (248), which contains Boetius et Beda de Computo, Mathesi, Astronomia, Geographia et vi ætatibus mundi. Haenel, 681. Unfortunately this account does not inform us which of the works are by Boethius. Is the Astronomia the same as that of the old Bobbio catalogue, and the Astrologia mentioned by Gerbert, in a letter written at Mantua probably in the year 972, "quod reperimus speretis, id est octo volumina Boetii de Astrologia, præclarissima quoque figurarum Geometriæ, aliaque non minus admiranda"?—Ep. 8. Cassiodorus alludes to a Latin translation of Ptolemy by Boethius, Var. i. 45.

‡ Ekkehard was born about A.D. 980 and died about A.D. 1036.—Arx in Pertz, Mon. Histor. t. ii. p. 74. Obbarius (p. xxxvii. *n.* 42) suggests that this may perhaps be one of the two MSS. of Boethius, bequeathed to the monastery of St. Gall and the abbot Hartmuth, in the last quarter of the ninth century (Ratpert. Cas. S. Gall. in Pertz. Mon. Hist. ii. p. 72, 45.). The words of Ratpert 'Boethii 5 libri philosophicæ consolationis in volum. i. Item alii 5 in altero volumine' seem rather to mean that he gave two copies of the same work. Compare p. 70, 33. And such was apparently the opinion of Arx, the learned librarian of St. Gall; for he has not noticed it among the books mentioned by Ratpert, which are still to be found in their ancient repository.

LXVI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 470.]

Jan. 17, 1850. RESEARCHES respecting the Molecular constitution of the Volatile Organic Bases. By Dr. A. W. Hofmann. Communicated by Sir James Clark, Bart., F.R.S.

Chemists, although all acknowledging the existence of an intimate relation between the vegetable alkaloids and ammonia, are nevertheless divided in their opinions respecting the nature of this connection, two theories having been propounded upon the subject. According to the one, that of Berzelius, the bases would have to be considered as conjugated ammonias in which ammonia still pre-exists as such; while according to Liebig's views, these substances are represented as amides, *i.e.* as ammonia in which one equivalent of hydrogen is eliminated and replaced by an equivalent of a compound radical.

The researches of the author prove that the theory of Berzelius is inadmissible, at all events for the volatile organic bases, inasmuch as in these substances ammonia ceases to exist as such. They show, moreover, that Liebig's view, although correctly expressing the constitution of by far the greater number of the volatile bases known, and presenting, when considered at the time it was first propounded, a wonderful anticipation of subsequent discovery, represents nevertheless only a special case of a much more general relation. The result at which the author has arrived is, *that ammonia is capable of losing either* 1 (*Liebig's case*) *or* 2 *or* 3 *equivs. of hydrogen which are respectively replaced by* 1, 2 *or* 3 *equivs. of the same, or various compound radicals, a variety of substances apparently endless being produced, in which its fundamental property* (*the basic character*) *is retained, although modified by the number of radicals introduced and their position in the scale of organic compounds.*

In support of this statement, he adduces the artificial production of thirteen new organic alkaloids, formed by a method which affords the means of increasing the number of these substances to an indefinite extent. This method consists in exposing ammonia to the action of the chlorides, bromides or iodides of the alcohol radicals, which remove 1 equivalent of hydrogen of the latter, as hydrochloric, hydrobromic, &c. acid, while the remaining constituents, assuming the alcohol-radical, give rise to the formation of an organic base which unites with the hydrogen acid.

By subjecting the new base itself to a similar treatment, another equivalent of the two remaining equivalents of hydrogen may be removed, a second organic base being formed, which in its turn gives rise to a third.

The changes which the ammonia undergoes in these various processes may be represented graphically by the following simple formulæ, X, Y and Z, denoting generally compound radicals.

$$\left.\begin{array}{l}H\\H\\H\end{array}\right\}N+XBr=\left.\begin{array}{l}H\\H\\X\end{array}\right\}N.HBr.$$

$$\left.\begin{array}{l}H\\H\\X\end{array}\right\}N+YBr=\left.\begin{array}{l}H\\X\\Y\end{array}\right\}N.HBr.$$

$$\left.\begin{array}{l}H\\X\\Y\end{array}\right\}N+ZBr=\left.\begin{array}{l}X\\Y\\Z\end{array}\right\}N.HBr.$$

For the illustration of these general formulæ, one of the numerous sets of experiments which the author has communicated in his paper may be quoted in which $X=Y=Z$. Ammonia, when exposed to the action of bromide of ethyl (hydrobromic ether), is converted into hydrobromate of ethylamine, *i.e.* ammonia in which 1 equivalent of hydrogen is replaced by ethyl, a compound which was first observed by M. Wurtz under perfectly different circumstances. Ethylamine, treated again with bromide of ethyl, yields a new alkaloid diethylamine, *i.e.* ammonia in which 2 equivalents of hydrogen are replaced by ethyl, and which, under the influence of a further quantity of bromide of ethyl, lastly is transformed into triethylamine, or ammonia in which the whole of the hydrogen is replaced by ethyl. This is a most powerful alkali, whose properties resemble those of caustic potassa.

Jan. 24.—Observations on the Freezing of the Albumen of Eggs. By James Paget, Esq., Professor of Anatomy and Surgery to the Royal College of Surgeons. Communicated by Thomas Bell, Esq., Sec. R.S. &c.

The object of this paper is to illustrate a peculiar property of the albumen of the eggs of birds, a property which seems to have its purpose in preserving them from the injurious effects of very low temperatures.

Mr. Hunter observed that a fresh egg will resist freezing longer than one which has been previously frozen and thawed; and he referred this fact to the 'vital power' of the egg in the first case, and the destruction of that power by freezing in the second. The author's experiments confirm those of Mr. Hunter, and prove, also, that when fresh eggs are exposed to very low temperatures, and also in the case of eggs which are decayed, or putrid, or the contents of which have been much altered by mechanical force or by electricity, a shorter time is sufficient for the freezing of such eggs, than is necessary for the freezing of those which are uninjured.

An examination of the rates at which heat was lost by the several eggs, exposed to temperatures varying from zero to 10° Fahr., showed that fresh eggs, though they resist freezing longer than any others, yet lose heat more quickly; and that their resistance to freezing is due to the peculiar property of their albumen, the temperature of which may be reduced to 16° Fahr., or much lower without freezing, although its proper freezing-point is at or just below 32°. Other than fresh eggs lose heat comparatively slowly, but freeze as soon as their temperature is reduced to 32°; fresh eggs lose heat more

quickly, but may be reduced to 16° or lower; then, at the instant of beginning to freeze, their temperature rises to 32°.

That this peculiarity of fresh eggs is not due to vital properties, is proved by experiments which show that certain injuries, such as mechanical violence, addition of water, and others, which spoil their powers of resisting freezing, do not prevent eggs from being developed in incubation. By the same and other experiments, which are related, it is made probable that the peculiarity depends on the mechanical properties of the albumen; for, whatever makes the albumen more liquid than it is naturally in the fresh egg, destroys the power of resisting freezing.

The author could find no other substance possessing this property; and in evidence of its adaptation to the purpose of preserving eggs from the loss of their capacity of developement, which they would suffer in being frozen, he relates experiments in which eggs were kept for a considerable time at temperatures ranging from zero to 10° Fahr., yet were afterwards developed in incubation. By the same series of experiments it was shown, that, although freezing renders the effectual developement of the germ impossible, yet the intensest cold, if freezing does not take place, has no similar result.

2. A Letter from M. Kupffer, to Lieut.-Col. Sabine, For. Sec. R.S., "On the establishment of a Central Physical Observatory at St. Petersburg." Communicated by Lieut.-Col. Sabine.

3. A Letter from Captain C. M. Elliot, Madras Engineers, to Lieut.-Col. Sabine, For. Sec. R.S., transmitted through the Court of Directors of the East India Company. Communicated by Lieut.-Col. Sabine.

Having undertaken the magnetic survey of the Indian Archipelago at the recommendation of the Royal Society, I think a slight sketch, detailed as briefly as possible, of my operations may not be uninteresting to Sir John Herschel and the Committee of Physics of which he is Chairman, prior to the publication of the Survey. I trust likewise I have acted strictly in accordance with the wishes of those who so kindly recommended me for the Survey, and I hope that my earnest efforts to do my duty will gain for me that approbation which I have under no ordinary difficulties incessantly striven to obtain.

I will in the first place mention the different stations I visited, and then describe in a few words, the way in which the observations were taken.

I have made a most complete survey of Java. At Batavia I established an observatory where observations, magnetic and meteorological, were taken hourly from 3 A.M. to 9 P.M. for nine months. In addition, about fifty stations, where observations of dip, of total intensity, of latitude, longitude, and declination were taken; these were always made by myself, and I am certain they can be depended upon.

In Borneo an observatory was established at Sarawak, where observations were taken quarter-hourly for three months, besides visiting

the Dutch settlements of Sambas, Pantianak and Succadana on the western coast.

In Sumatra four months of observation at Padang, besides a magnetic survey comprising about thirty stations. I crossed the equator here as well as at Pantianak in Borneo.

At Singapore I compared the portable instruments with the fixed instruments of the observatory, besides determining the horizontal intensity and dip, which had not been accurately determined previously from insufficiency of means.

At the Cocos or Keeling Islands, six weeks of observation.

At Sambooangan, in the Island of Mindanao, upwards of a week.

At Keemah in Celebes, the same.

At Penang, the same.

At Moulmein, the same.

At the Nicobars, the same.

At Bencoolen in Sumatra, the same.

I will now mention the instruments and the mode of observation at the observatories. The following instruments were registered every hour from 3 A.M. to 9 P.M. Two declinometers, and latterly a third made by Jones; a bifilar magnetometer and its thermometer; a standard barometer and its thermometer by Newman; a standard thermometer and a dry and wet bulb.

On the survey, I employed for the observations four dipping-needles; a portable declinometer with altitude and azimuth instrument for the declination and for latitude; a sextant, artificial horizon and chronometer for the error and rates of the watch, which was but a poor one.

I began work generally at 6 A.M., put up the portable declinometer, and allowed the brass weight and stirrup to swing for a couple of hours thoroughly to take the torsion out of the thread, adjusting it from time to time so that the stirrup might ultimately take up a position in the magnetic meridian; during this period I took the dip. At 8 A.M. I took sights with the sextant, and putting in the collimator magnet, I adjusted the altitude and azimuth to it, and took altitudes and azimuths of the sun, three on the limb direct, and three on the limb reversed, noting the reading on the horizontal limb; at the same time this gave me the reading of the true meridian; the magnetic axis of the collimator magnet was then read off; these observations were usually completed by 9½ A.M.: by 11 A.M. I had finished my observations for horizontal intensity. The small collimator magnet being suspended, the large collimator magnet was placed at four different distances east and west, and the deflecting collimator magnet was then vibrated and 300 oscillations taken.

At 11 A.M. I observed altitudes and azimuths of the sun with the altitude and azimuth instrument for equal altitudes. At noon I took circummeridional altitudes of the sun, three with the limb direct, and three with the limb reversed, for latitude; at 1 P.M. I again took altitudes and azimuths.

By equal altitudes from the mean of the times, I was enabled to check the results given by the sextant for time; and by the azimuths corresponding to the equal altitudes, I checked the observations for

the true meridian taken at 9 A.M. for the declination. By this means I was always certain of the results by using different modes of verification.

If I stopped another day at the stations I repeated the observations; if I was going to move off, I packed up the instruments and struck the tents, which generally took me the afternoon and the greater part of the evening, for I had no one to assist me.

At sea, whenever an opportunity offered, I took meteorological observations, viz. the standard thermometer, the dry and wet bulb, and the temperature of the air and sea at 3 A.M. and P.M., and at 9 A.M. and P.M.; sights for longitude at 9 A.M. and 3 P.M., and latitude at noon, besides the dip three times, and sometimes five times a day; every absolute determination was made by me.

Thus on shore as well as at sea, observations were commenced at 3 A.M., and never terminated till 9 P.M.: I had for my assistant an Indo-Briton.

I will not trouble the Council of the Royal Society with stories of the difficulties I met with; suffice it to say, that a stranger amongst the Dutch, the necessity of conciliating the natives in seeing me employed in a manner so strange to them, travelling in the monsoons and in all weathers, sometimes for hours wet in the saddle, living in huts for weeks, the only shelter being cocoanut leaves, and at sea in a leaky old schooner that was perpetually in danger of foundering, with a captain who was scarcely ever sober,—it is not surprising that once or twice I fell sick. I am now but slowly recovering from Java and Car Nicobar fever caught in the execution of my duty. I take the liberty of adding for the information of the Council of the Royal Society, that I never took a single observation unless I was by myself and my attention undisturbed. If strangers were importunate, I waited until they left me. If the weather was against me, I took no observations until it settled. I made it a rule never to be in a hurry, and always to finish one set of observations before I commenced another, and to be as comfortable as circumstances would admit. I am certain that an observation is the more valuable in proportion to the mind being not only at ease, but able to fix itself with undivided attention on the observations. I never, for instance, would think of taking an observation whilst bored by an intruder, or a high wind, or a heavy shower of rain falling. I preferred under such circumstances invariably to sacrifice the observation rather than to record it.

I have the honour of sending to the Council as a specimen of the way in which the work was carried on, some of the absolute determinations made at the Cocos or Keeling Islands; they will be able to see that often after the labours of the day had commenced at 3 A.M., they were not terminated at 9 P.M.; and in order to observe moon-culminating stars, I had sometimes to remain up the greater part of the night, for I had no one on whom I could place any dependence to awake me at the proper time.

Paper A contains the way and order in which the instruments of the observatory were registered.

Paper B. The dip taken at the Cocos, with an example.

Paper C. The horizontal intensity, with an example.
Paper D. The declination, with an example.
Paper E. The latitude, with an example.
Paper F. The longitude from moon-culminating stars.

I have the honour to be, Sir,
Your most obedient Servant,
C. M. Elliot,
Captain Madras Engineers.

Madras,
August 6, 1849.

Jan. 31.—"An account of a remarkable Aurora Borealis seen at Montreal on the 13th of August 1849." By Mr. Thomas McGinn. Communicated by Thomas Bell, Esq., Sec. R.S., &c.

The author having witnessed a singular aurora on the 13th of August, in this communication gives a description of the phænomenon. He states that, on the evening in question, the whole northern hemisphere was screened by thick dark clouds, which, though very small, were closely packed together. Shortly after sunset (7^h 34^m) it became quite dark, and at 8 o'clock the existence of the meteor was indicated by a mellow luminous tinge which appeared through the openings of the clouds in the north.

About half-past eight a similar luminous glow was observed through the clouds which were fast disappearing in a heavy dew. This light appeared like a belt of 2° broad, extending across the sky from a point almost due east directly to the west, and reaching within 5° or 6° of the horizon. As the clouds disappeared, which they did very rapidly, the true character of the aurora became more perfectly developed. In the north the usual dark arch from which the columns of light ordinarily appear to issue, was for the greater part of the time wanting; and the luminous columns seemed to rise from the earth, extending upwards occasionally to the pole star, beyond which no trace of them was visible. A brown vapoury cloud, the only one now visible, extended along the horizon from N.N.E. to a few points south of east, and maintained apparently a motionless position, the lower part appearing to rest upon the earth, and the upper edges, which seemed uniform, rose about 6° above the horizon. Immediately in the east, and apparently issuing from this cloud, rose the belt or zone of light already noticed, forming a magnificent arch. The light emitted from this zone was of a milky whiteness, and the matter of it seemed to be much more compact than any portion of an aurora ever seen by the author; but immediately in the zenith, where it intersected the Milky Way, it appeared to be far less compact. At this point, where alone motion was observable, a constant current was seen, presenting the appearance of light fleecy clouds driven by a strong wind, and following each other in such close succession as to appear in contact. This stream of the aurora was maintained undiminished for more than an hour, during which time the eastern part of the zone did not appear to lose either in volume or brilliancy, nor did the western seem to gain in either of these respects. After an hour, the dark cloud seemed to diminish slowly, and with it the zone began to lose its brilliancy. In about another hour this cloud and also the zone,

which throughout had maintained apparent contact with it, vanished. The conclusion, that the dark cloud served the purpose of a conductor and fed the zone drawing off the matter of the aurora from the north, seemed to the author inevitable. The cloud did not appear to him to be more than forty or fifty miles distant. In conclusion he remarks that none of the prismatic tints were observable on this occasion.

2. "On the Development of the Retina and Optic Nerve, and of the Membranous Labyrinth and Auditory Nerve." By Henry Gray, Esq., M.R.C.S. Communicated by William Bowman, Esq., F.R.S., &c.

The author has divided the observations contained in this paper into two parts:—the first of which treats of "The Development of the Retina and Optic Nerve; the second, of the Development of the Membranous Labyrinth and Auditory Nerve."

In the observations on the development of the retina, which have been made on the embryo of the chick, the author demonstrates its mode of evolution, and also the mode of development of the various layers of which this membrane is formed. They commence at the early period of the thirty-third hour of incubation, at which time the cephalic extremity of the embryo presents a slight protrusion of its walls, which by the thirty-sixth hour is very considerably increased, having become more elongated and protruded outwards, presenting a somewhat dilated end, and being somewhat constricted at its connection with the anterior cerebral cell from which it arises. This is the first indication of the development of this membrane.

At the forty-sixth hour this protrusion (which the author calls the optic vesicle) was still more distinct, and the cavity in the cerebral cell, from the wall of which it arises, was well seen, and it was observed to communicate with the cavity of the optic vesicle which was also hollow. This description of the mode of development of the retina the author considers as confirmatory of the observations made by Baër, but not in accordance with that given by Wagner or Huschke.

The author then proceeds to detail very minutely the consecutive stages of the development of the retina and parts in immediate connection with it, until the seventh day, when he states that on making a section of the eye, it was separated from the other tunics as a perfectly distinct layer. The optic nerves were also now completely formed, being united to form the chiasma, and passed inwards in the direction of the under surface of the corpora quadrigemina.

The author in the next place proceeds to consider the development of the various layers of the retinal membrane, a point which appears not to have been previously noticed by any physiologist. This membrane on the eighth day of incubation can be seen, by the naked eye, distinct from the other tunics. Its choroidal surface is composed of a mass of globular nuclei about the size of the red corpuscles of the blood, which form at this period about one-half the entire thickness of the retina, the deeper surface consisting of some fine granular matter and a mass of pale and delicate nucleated cells

similar to those found surrounding the fibrous lamina in the normal structure of the membrane.

The "Membrana Jacobi" is first observed on the thirteenth day as a fine pale granular stratum which covers in the globular nuclei already described. In this, at about the fifteenth day, some brilliant yellowish granules are imbedded; they vary in size from the 5000th to the 8000th of an inch, and around them a delicate cell-wall is traced; they soon acquire an oval shape; then become more elongated; and about the eighteenth day the almost perfect rods are formed. They are now disposed in an imbricated manner, and their nuclei, which are of a bright yellow colour, are placed generally at the apex, but sometimes in the middle of the rods. On the twenty-first day this membrane is similar to what is seen in the full-grown bird.

The first trace of the "fibrous lamina" is seen between the fourteenth and fifteenth days, as a fine pale granular lamina marked by numerous faint longitudinal striæ. On the eighteenth day this membrane when separated from the other layers is seen composed of numerous fibrillated meshes, in which are deposited the nucleated vesicles which are formed as early as the eighth day. From these observations it is seen that the retina is formed as a protrusion from the most anterior cerebral cell, being hollow and communicating with its cavity; that it subsequently assumes a pyriform shape, presenting a dilated end, the future retina, and a tubular portion, the optic nerve. As the tubular portion becomes solidified so as to form the optic nerve, then no communication can be traced between the optic vesicle and the cavity from which it is an offset. By degrees the spherical end of the protrusion is absorbed, and the retina, being now fully formed, becomes attached to the margin of the lens. The optic nerve is then traced to be connected not only with the anterior cerebral cell, but, uniting with its fellow at the under surface of the optic lobes, is seen partly to terminate in those bodies. The deductions from these observations may be thus briefly stated:—

1st. They confirm the observations on the structure of the retina made by Bowman, who has shown that the essential part of this membrane is analogous to the cineritious matter of the brain, and is composed like it of a fibrous mesh intermingled with vesicles of grey matter, being, in fact, a portion of the cerebrum pushed outwards and connected with the brain by its appropriate commissure, the optic nerve. The mode of development of this membrane would show this to be the correct view of the structure of this essential part of the retinal expansion, and at the same time disprove the statements of Henle, who believed it to be more analogous to epithelium.

2nd. The origin of the optic vesicle from the anterior cerebral cell, would show the incorrectness of the opinion of those anatomists who have stated that none of the fibres of the optic nerve could be traced to the optic thalami. The thalami being developed from the same centre from whence these vesicles arise, would render it exceedingly probable that the optic nerves had some connection with those bodies.

The second part of the paper describes the development of the membranous labyrinth and auditory nerve.

The essential part of the auditory apparatus, viz. the membranous labyrinth, consists, like the retina, of a membranous lamina formed of the terminal axes cylinders of the nerve tubules in intimate connection with a layer of closely-set nucleated cells; like it also, it may be regarded as a portion of the brain protruded outwards, and connected with an appropriate apparatus which receives and transmits its peculiar impressions; its mode of development also shows a striking analogy between it and the retinal expansion.

At the fiftieth hour of incubation, there is seen on either side of the medulla oblongata, (which is not closed in above and presents an open shallow cavity,) the first rudiment of the auditory sac, in the form of a small circular-shaped protruded vesicle, communicating with the ventricular cavity from the lateral wall of which it is an offset. The vesicle was hollow, clear and pellucid, and of a flattened circular form. At the fifty-sixth hour it had increased in size and presented a pear-shaped figure; so that now the narrow contracted tubular portion appeared the first stage in the development of the auditory nerve; the dilated portion, the auditory sac or rudimentary vestibule; and the cavity still existing in its interior and communicating with the ventricular cavity from which it arises, by means of the tubular prolongation, the auditory nerve. The aperture of communication soon becomes smaller and more contracted, and this increases as the separation between the auditory vesicle and its parent-cell takes place. At the sixty-fifth hour, besides a great increase in the size of the ear-bulb, the auditory nerve has become more distinctly formed, and is quite solidified, so that no communication can now be traced between the ventricular cavity and the vestibular sac. It is in this stage of the development of the auditory apparatus that a great similarity is to be observed between it and the normal condition of the same part in some of the lower animals. There are, in fact, now formed the two elementary portions of the auditory apparatus, the auditory nerve and the simple vestibular sac. Such is the simple condition of the organ in the Crustacea and Cephalopod Mollusks. At the seventy-second hour, the vestibular sac has lost its oval form and presents a contraction around its entire circumference. This is the first indication of the separation of the vestibule from the membranous semicircular canals which are ultimately formed from the terminal portion of the vesicle.

The minute examination of the development of these structures, of which a consecutive detail is given, leads the author to remark on the almost precise similarity in structure of the membranous labyrinth to the retina in its various stages of development, for it consists like it of a delicate fibrous mesh in the areolæ of which is deposited granular matter and numerous nucleated cells, its outer surface being composed of globular-shaped nuclei arranged similar to those covering the outer surface of the retina at an early period of its development.

From this description a marked similarity may be observed be-

tween the origin of this membrane and that of the retina. In both cases they arise as a protruded portion of the cerebral mass, being hollow and communicating with the cavity of the parent-cell. In process of time, a gradual separation takes place between them and the parts from whence they arise. They then assume a pyriform shape, but still communicate with the cerebral cavity. As, however, the nerves become solidified and the separation between them is more fully effected, then no communication can be traced between the two cavities.

3. "Tide Researches. Fourteenth Series. On the Results of continued Tide Observations at several places on the British Coasts." By the Rev. W. Whewell, D.D., F.R.S. &c.

Tide observations made at several different parts of the British and neighbouring shores, and in some instances continued for a considerable period, having been discussed by Mr. D. Ross of the Hydrographer's Office, with great labour and perseverance, a brief statement of the results which his labours afford is here presented by Dr. Whewell.

The discussions here referred to relate to the height of high water, and the variations which this height undergoes in proceeding from springs to neaps, and from neaps to springs. It is found, by examining the observations at 120 places, and throwing the heights into curves, that the curve is very nearly of the same form at all these places. Hence the semi-mensual series of heights at any place affords a rule for the series of heights at all other places where the difference of spring height and neap height is the same. For instance, Portsmouth, where the difference of spring height and neap height is 2 ft. 8 in., is a rule for Cork, Waterford, Inverness, Bantry, Arcachan on the French coast, and other places: and the tables of the heights of high water at one of these places suffices for all the others, a constant being of course added or subtracted according to the position of the zero-point from which the heights at each place are measured.

The series of heights of high water for a semi-lunation also agrees very exactly, as to the form of the curve, with the equilibrium theory. A very simple construction is given for the determination of this curve. The properties deduced according to theory from this construction are, however, in actual cases, modified in a manner which is then described.

1. The tides in these discussions are not referred to the transit of the moon immediately preceding, but to some earlier transit, namely, the second, third, fourth or fifth preceding transit, it being found that in this way the accordance with the theory becomes more exact.

2. According to this construction, the difference of springs and neaps would be to the height of neaps above low water springs as 10 to 24, a constant ratio for all places; but in fact this ratio is different at different places: and the observations under consideration show that the ratio is smaller where the tide is smaller.

In consequence of the law of the high water, given alike by the theory and by the observations, the spring high waters are above the mean high water for a longer period than the neaps are below it.

LXVII. *Intelligence and Miscellaneous Articles.*

ON THE APPLICATION OF ELECTRO-MAGNETISM AS A MOTIVE POWER. BY MR. ROBERT HUNT.

AT a recent meeting of the Society of Arts, the author called attention, in the first place, to the numerous attempts which have been made to apply electro-magnetism as a power for moving machines, and referred to the apparatus employed by Jacobi, Dal Negro, M'Gauley, Wheatstone, Hjorth, and others. Since, notwithstanding the talent which has been devoted to this interesting subject, and the large amount of money which has been spent in the construction of machines, the public are not in possession of any electro-magnetic machine which is capable of exerting any power œconomically; and finding that, notwithstanding the aid given to Jacobi by the Russian government, that able experimentalist has abandoned his experimental trials, the author has been induced to devote much attention to the examination of the first principles by which the power is regulated, with the hope of being enabled to set the entire question on a satisfactory basis.

The phænomenon of electro-magnetic induction was explained, and illustrations given of the magnetization of soft iron by means of a voltaic current passing around it. The power of electro-magnets was given, and the author stated his belief that this power could be increased almost without limitation. A voltaic current produced by the chemical disturbance of the elements of any battery, no matter what its form may be, is capable of producing by induction a magnetic force, *this magnetic force being always in an exact ratio to the amount of matter (zinc, iron, or otherwise) consumed in the battery.*

Several forms of the voltaic battery were explained, particularly those of Daniell, Grove, Bunsen and Reinsch, the latter being constructed without metals, depending entirely on the action between two dissimilar fluids, slowly combining.

The author had proved, by an extensive series of experiments, that the greatest amount of magnetic power is produced when the chemical action is the most rapid. Hence, in all magnetic machines, it is more œconomical to employ a battery under an intense action than one in which the chemical action is slow. It has been proved by Mr. Joule, and most satisfactorily confirmed by the author, that one-horse power is obtainable in an electro-magnetic engine the most favourably constructed to prevent loss of power, by the consumption of 45 lbs. of zinc, in a Grove's battery, in twenty-four hours; while 75 lbs. are consumed in the same time to produce the same power in a battery of Daniell's construction. The cause of this was referred to the necessity of producing a high degree of excitement, to overcome the resistance which the molecular forces offer to the electrical perturbations on which the magnetic force depends.

It was contended, that although we have not perhaps arrived at the best form of voltaic battery, yet that we have learned sufficient

of the law of electro-magnetic forces to declare, that, under any conditions, the amount of magnetic power would depend on the change of state, consumption of an element, in the battery, and that the question resolved itself into this, What amount of magnetic power can be obtained from an equivalent of any material consumed? The following were regarded as the most satisfactory results yet obtained:—1. The force of voltaic current being equal to 678, the number of grains of zinc destroyed per hour was 151, which raised 9000 lbs. 1 foot high in that time. 2. The force of current being relatively 1300, the zinc destroyed in an hour was 291 grs., which raised 10·030 lbs. through the space of 1 foot. 3. The force being 1000, the zinc consumed was 223 grs., the weight lifted 1 foot 12,672 lbs.

The estimations made by Messrs. Scoresby and Joule, and the results obtained by Œrsted, and more recently by Mr. Hunt, very nearly agree; and it was stated that 1 gr. of coal consumed in the furnace of a Cornish engine, lifted 143 lbs. 1 foot high, whereas 1 gr. of zinc consumed in the battery lifted only 80 lbs. The cost of 1 cwt. of coal is under 9 pence, the cost of 1 cwt. of zinc is above 216 pence. Therefore, under the most perfect conditions, magnetic power must be nearly 25 times more expensive than steam power. But the author proceeded to show that it was almost proved to be an impossibility ever to reach even this condition, owing, in the first place, to the rate with which the force diminishes through space. As the mean of a great many experiments on a large variety of magnets, of different forms and modes of construction, the following results were given:—

The magnet and armature being in contact, the lifting force was . . . . 220 pounds.

...	distant $\frac{1}{250}$ of an inch	90·6	...
...	$\frac{1}{125}$	50·7	...
...	$\frac{1}{63}$	50·1	...
...	$\frac{1}{50}$	40·5	...

Thus at one-fiftieth of an inch distance four-fifths of the power are lost. This great reduction of power takes place when the magnets are stationary. The author then proceeded to show that the moment they were set in motion a great reduction of the original power immediately took place; that, indeed, any disturbance produced near the poles of a magnet diminished, during the continuance of the motion, its attractive force; the attractive force of a magnet being 150 lbs. when free of disturbance, fell to one-half by occasioning an armature to revolve near its poles. Therefore, when a system of magnets which had been constructed to produce a given power is set in revolution, every magnet at once suffers an immense loss of power, and consequently their combined action falls in practice very far short of their estimated power. This fact has not been before distinctly stated, although the author is informed that Jacobi observed it. And not merely does each magnet thus sustain an actual loss of power, but the power thus lost is converted into a new

form of force, or rather becomes a current of electricity, acting in opposition to the primary current by which the magnetism is induced.

From an examination of all these results, Mr. Hunt is disposed to regard electro-magnetic power as impracticable, on account of its cost, which must necessarily be, he conceives, under the best conditions, 50 times more expensive than steam power.

The chairman agreed with Mr. Hunt in his conclusion of the improbability of any result being obtained from electro-magnetism which could enable it to compete with steam as a motive power. At any rate, the point to which the attention of engineers and experimentalists should be turned at present was, not the contriving of perfect machines for applying electro-magnetic power, but the discovery of the most effectual means of disengaging the power itself from the conditions in which it existed stored up in nature. Mr. Faraday assured us that in a single drop of water is contained as much electricity as is developed in a thunder-storm. The portion of this which we can liberate by any existing battery is very small, so small, that, as shown by Mr. Hunt's paper, its practical use cannot be profitable. The study of electro-chemistry, he thought, was a more promising field, and one from which might at a future time be developed a power which should supersede even steam.

FIBROUS HYDRATE OF MAGNESIA, NEMALITE OF NUTTALL, THOMSON AND CONNELL.

The fibrous hydrate of magnesia, which was first discovered and named by Nuttall without analysis, but which was considered by him as hydrate of magnesia, has been twice subjected to analysis with very discordant results. Thomson, having examined a specimen which contained a portion of silica, or silicate of magnesia, mechanically intermixed, gave for this mineral the formula

$$MgO,SiO^3+2MgO,HO,$$

his analysis having given him about 12 per cent. of silica. Connell has more recently analysed the same mineral, and, happening to have a specimen which contained no silica, but a considerable quantity of carbonate of magnesia, also mechanically intermixed, he gives as the result of his analysis the formula

$$5MgO,HO+MgO,CO^2+HO,$$

a highly improbable one.

Mr. Whitney has examined a specimen of this mineral from the cabinet of F. Alger, Esq., and finds that when perfectly pure it contains neither silica nor carbonic acid, but that it is a fibrous hydrate of magnesia, though it often occurs mixed with the silicate and carbonate of magnesia. If a few fine fibres of the mineral be placed in dilute acid, the effervescence will be found to be but momentary, and confined to the extremities of the fibres, where they were in contact

with the gangue; as soon as the adhering impurities have been removed, the mineral dissolves without effervescence.

The following results of an analysis will show conclusively that the nemalite is essentially hydrate of magnesia, or brucite, from which it does not differ otherwise than by being in a fibrous state:—

Magnesia	62·89
Protoxide of iron	4·65
Carbonic acid	4·10
Water (by loss)	28·36
	100·00

A small portion of magnesia is replaced by protoxide of iron. The formula of brucite, MgO, HO, requires—

Magnesia	69·67
Water	30·33
	100·00

Boston Journal of Natural History, vol. vi. p. 36.

ANALYSES OF PECTOLITE AND STELLITE, AND PROPOSED UNION OF THESE TWO SPECIES. BY J. D. WHITNEY.

Pectolite occurs on Isle Royale, Lake Superior, in spheroidal masses, consisting of delicate silky fibres radiating from a centre, which exactly resemble the foreign specimens of this mineral from Monte Baldo. The radiated stellated mineral from Bergen Hill, New Jersey, which was analysed by Beck, and supposed by him to be identical with the stellite of Thomson, agrees also in external characters with the pectolite. Specimens from Isle Royale and from Bergen Hill fuse, like pectolite, readily, with but little intumescence, to a colourless glass. They are easily dissolved by hydrochloric acid, the silica separating as a flocky powder.

The following are the results of the analysis of specimens of the pectolite and stellite:—

	I.	II.	III.	IV.
Silica	53·45	55·66	54·00	55·00
Lime	31·21	32·86	32·10	32·53
Soda	7·37	7·31	8·89	9·72
Potash	trace	..	trace	
Alumina	4·94	1·45 MnO	1·90 Mn	1·10
Water	2·72	2·72	2·96	2·75
	99·69	100·00	99·85 J.S.K.	101·10 G.J.D.

I. and II. are specimens from Isle Royale. No. I. contains a considerable portion of alumina, which is evidently not essential to the composition of the mineral, since II., resembling it entirely in external appearance, gives only $1\frac{1}{2}$ per cent. The silica in both these

analyses contained a small quantity of substance insoluble in carbonate of soda, evidently quartz, mechanically intermixed with the finely-fibrous mineral.

III. is the mineral from Bergen Hill, New Jersey, analysed by Beck. He has erroneously given 6·8 per cent. of magnesia in this mineral. Otherwise, substituting soda for magnesia, his analysis agrees pretty nearly with the one given above, which was done under my direction by Mr. J. S. Kendall. Hayes had also analysed this mineral, and corrected Beck's analysis as far as relates to the absence of magnesia and the presence of soda. He however did not find that it contained water, which is essential to the composition of pectolite.

IV. is also a fibrous mineral from Bergen Hill, which evidently agrees in composition with pectolite. It differs from the other specimen from the same locality in its fibres being straight, and not grouped together into star-like forms. This analysis was executed at my request by Mr. G. J. Dickinson.

It is evident that these minerals all agree in chemical composition with the pectolite of Von Kobell, and also in external characters. Slight differences in the results of the analyses may easily be accounted for by the difficulty of procuring a finely fibrous mineral in a state of known freedom from intermixture with foreign substances.

The formula given by Von Kobell for pectolite is—

$$3(NaO, 3SiO^3)+4(3CaO, 2SiO^3)+3HO,$$

which formula requires—

Silica	52·55
Lime	34·94
Soda	9·70
Water	2·79
	99·98

Frankenheim considers the water in the pectolite as unessential, and allies this mineral with the augite family, from which it differs widely in chemical characters. The constant presence of nearly 3 per cent. of water in all the analyses of the substance dried at 100° C., makes it highly improbable that it should be merely accidental. In fact, the formula given above seems to be the only one which could be adopted for this mineral.

The original stellite, described by Thomson as occurring in Scotland, was probably an impure specimen of pectolite, which mineral it agrees with in external characters, as well as in chemical composition, merely substituting soda for magnesia. The mineral described by the same chemist under the name of Wollastonite, under the erroneous impression that that name had not been generally adopted for table-spar, is also evidently identical with pectolite.—*Boston Journal of Natural History*, vol. vi. p. 40.

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END OF THE THIRTY-SIXTH VOLUME.

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ALERE FLAMMAM.

Fig. 1.

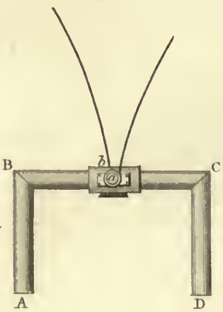

Fig. 2.

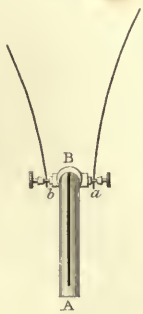

Fig. 5.

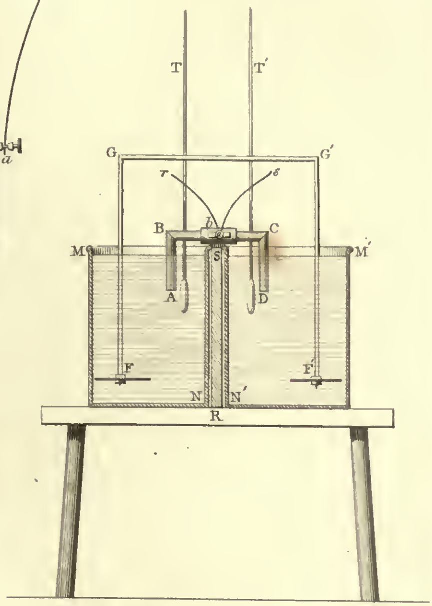

Fig. 3.

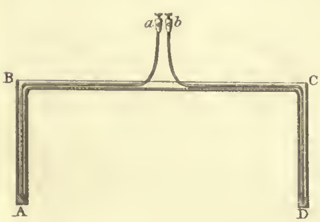

Fig. 4.

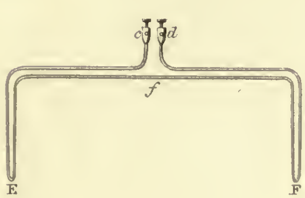

Fig. 6.

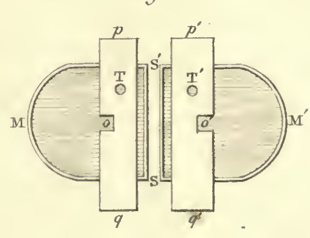

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Fig. 1.

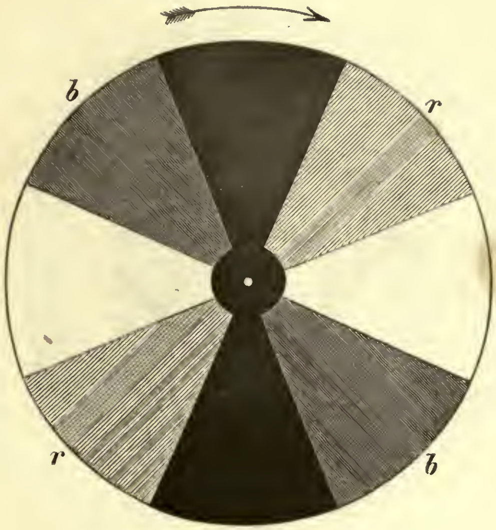

Fig. 2.

Fig. 1.

Fig. 2.